

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII.

Supplementary Notice.

No. 9.

Note accompanying a Chart of those Antarctic and Sub-Antarctic Regions which are suitable for observing the Transit of Venus in 1874. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

To complete the processes of charting which I have thought it desirable and necessary to undertake in connexion with the Transit of 1874, I now present a chart of the regions where the duration of the transit will be considerably shortened.* It will be observed that the map includes every point on the Earth's surface where the duration of the transit will be less than the mean duration by eight minutes, the Sun not being less than ten degrees high both at ingress and egress (internal contacts).

The chart requires no explanation beyond perhaps the remark that the islands in the less known regions have been taken from ordinary atlases (after comparison of several), in preference to the Admiralty charts; because, after certain withdrawals from opinions expressed in December 1868, one naturally feels doubtful about Admiralty statements, which would appear to be variable according to official requirements. It did not seem well to insert any island, or group of islands, in the chart, with some such note as, "Here, if convenient to those in authority, there is an island," or "This

* To prevent misapprehension I feel it desirable to mention that none of the charts in this number have been engraved at the expense of the Society. This remark applies to Mr. Sidney Waters's beautiful and most valuable charts. As the Supplementary Number is the only one which does not receive the direct sanction of Council, the object of this note will be recognised, and no doubt its necessity will be perceived, by all Fellows resident in Great Britain whose election dates from before last February.

group of islands can be regarded as a reality or a myth, as may be required," and so on.

It will be perceived from the chart that Macdonald, or Heard Island (the only new observing station suggested in response to the advice of the Greenwich Board of Visitation), although well placed, is somewhat too near to Kerguelen Island to have favourable independent prospects of good weather. In other words, the occupation of this island will be useful, as increasing the number of Southern Halleyan stations and the value of the Southern observations as a whole, supposing weather to be favourable; but it is not a station which adds greatly to the probability of success, so far as success depends on conditions of weather.

I cannot conclude the statement of my views respecting the approaching transit without renewing the expression of regret that the transit of 1874 should not have been correctly viewed from the beginning by the persons responsible for England's action in astronomical matters. We do not know precisely what would have happened in that case; though we can infer from what was (mistakenly) suggested as proper for the transit of 1882, that a course would have been pursued which could not but have reflected credit on this country, both as respects scientific zeal and the spirit of enterprise. That an unfortunate mistake, admitted too late, should have led to such an anomalous state of things that British official astronomy and British nautical authority find it necessary, "with a common understanding, and therefore with considerable effect," to make little of opportunities whose importance they once fully and publicly recognised (in a precisely corresponding case), is grievously to be regretted. To the earnest student of science it must also be a cause of serious regret that such opportunities should be wasted. But while as an Englishman and as a student of science I must needs share in these regrets, those mistake who imagine that in a personal sense I regard with any feeling but the most complete indifference the action of persons responsible for advising our Government in this matter. *Whatever justification my researches and appeals may have seemed to require has been afforded by the unanimous vote of the leading British astronomers assembled at the Greenwich Board of Visitation.* Those astronomers doubtless feel, as I have long felt, that it is *hereafter*, and not perhaps till many years hence, that final judgment will be formed on the matters which have been under discussion. Such judgment will be based, not on the skill with which official influence may have been exercised to check the action of a too profusely liberal government, but on the care and accuracy with which the questions at issue have been weighed in their scientific aspect. Posterity, in considering the course of the astronomers of this country, will inquire whether their advice was sound, not whether it received due attention—a point which can only affect those responsible for conveying such advice to the proper quarter. *In*

fine, I apprehend that in after years it will be thought worthier to have indicated, even if ineffectually, the proper course, than to have adopted effectual measures to prevent the proper course from being carried out.

Further Notes on Star-Gauging, and on the Principles on which its Interpretation should depend. By R. A. Proctor, B.A. Honorary Fellow of King's College.

From communications which have reached me, I am disposed to believe that some misapprehensions probably exist respecting the nature and objects of the processes of star-gauging which I have suggested as forming at present the best available means for determining the laws according to which the stellar universe is constituted. I therefore add some remarks which may be of use in explaining my views.

First, the great object of star-gauging with any given instrument is to ascertain how many stars *that instrument* shows in each portion of the heavens,—when the sky is clear and dark, and there is no twilight, moonlight, or other cause of variation in space-penetrating power. Great exactness in enumeration is by no means necessary. In fact, the effort to secure great exactness would certainly, by extending the time necessary for the work of survey, defeat the whole object of the work. What is required is a complete but rapidly effected survey, bearing the same relation to the actual charting of stars, that the reconnaissance of a land region bears to trigonometrical survey.

In interpreting results, attention must be directed, *first*, to the numerical distribution indicated by each particular instrument, such distribution being presented to the eye by the process of equal-surface charting; and, *secondly*, to the differential results indicated when a survey by any given instrument is compared with a survey by another instrument of greater or less space-penetrating power.

Unless both these orders of indication are considered, the star-gauging can afford no satisfactory information. This will be manifest if it be remembered that according to my views,—and I think I may say, according to the results I have demonstrated by equal-surface charts already constructed,—the great difficulty in all researches into the construction of the heavens consists in distinguishing the effects of star-distribution resulting, on the one hand, from great extension in the line of sight with consequent great variation in the apparent magnitude of stars of the same order, and on the other hand from the aggregation of many orders of real magnitude within one and the same region. These, of course, are only the extreme cases, and in nearly every instance, if not absolutely in every instance, both causes of varying apparent magnitude and closeness of aggregation are effective.

It is absolutely impossible in my opinion to distinguish one from the other, or to ascertain in what degree one or other operates, except by the proposed method of star-gauging, which combines, be it noticed, the qualities of the two methods suggested by Sir W. Herschel (one in 1784, the other in 1817), without being liable to the causes of error which exist in either of those methods. The general principles of interpretation, applicable not only to star-gaugings with different instruments, but to all differential observations, whether relating to colour, to duplicity triplicity or grouping generally, to spectrum, to motions whether thwart or in the line of sight, to peculiarities of configuration, to the various orders of nebulae, or so on, are simply these,—

(i.) Where two orders of observation indicate concordant laws of distribution over the heavens, the rich regions so indicated are regions of SPACE where the two orders of objects are intermingled so as to form parts of one system.

(ii.) Where the results are in direct contrast (or discordant in the true sense of the word*), the rich regions for one order corresponding to the poor regions for the other, and vice versâ, the two orders of objects belong to one system, but some peculiarity in their nature, or in the laws according to which they were formed, causes them to occupy different parts of the system, segregating as it were from each other.

(iii.) Where no connexion whatever, either of agreement or contrast can be observed, it is probable, and in general presumable, that the two orders are altogether distinct and lie at different distances from each other.

And, lastly (iv.), whether partial or local agreement or contrast is indicated, then the inference is that the true arrangement of the objects in space is affected both by laws of aggregation or segregation and by diversities of distance, and by one cause or the other to a degree indicated by the extent of such agreement or contrast.

Note on the Expulsion Theory of the Solar Corona, Comets, &c., with special reference to Prof. Norton's original enunciation of that Theory. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

I have for some time intended to present to the Fellows of this Society a *résumé* and discussion of the papers of Prof. Nor-

* Which, however, is ordinarily used in too vague a way to be suitable as the converse of concordant.

ton, of New Haven, Conn., U.S., on the subject of the theories respecting the corona, comets, &c., recently advocated by myself, —partly with the intention of indicating his claim to priority, and partly because of the intrinsic interest and importance of his remarks. But a great and increasing pressure of engagements has prevented me from doing so in a permanent form, though I have on several occasions indicated the state of the case in lectures.

I must now leave to another opportunity and another place the preparation of a full account of Prof. Norton's views; but it appears to me desirable to indicate his claim to priority, because it was in these pages that I first advocated the theory (which, however, I have not described as mine, having, in fact, distinctly stated that it was *adopted and advocated, not conceived*, by me.)

In a letter to me, Prof. Norton writes as follows: "The principal feature of the auroral theory which I have advocated is that the corona is made up of matter in the act of streaming off from the Sun, instead of being a permanent solar atmosphere, or a mass of revolving meteors, and this is precisely the great distinguishing feature of your theory that the corona is a phenomenon of eruption. The theory of an outstreaming of solar matter I propounded in the second edition of my *Astronomy*, published in 1845. I there say, 'The explosive actions which are the most probable causes of these spots, may perhaps furnish the luminary matter, which may afterwards be driven off to an indefinite distance by some repulsive action of the Sun. Certainly if there is at the Sun's surface any matter of the same nature as that of which the tails of comets are composed, it must be expelled by the same repulsive force that drives off this species of matter from the heads of comets and forms their tails.' The auroral theory of the corona is distinctly set forth in the edition published in 1867. The outstreaming of solar matter is a prominent feature of it, as appears from the following paragraph. 'The photospheric matter, dispersed, by reason of the varying action of the planets, sufficiently to become subject to a repulsive action from the Sun, as it flows away into space, forms the *corona*, with its accompanying radiations and streamers, visible in total eclipses.' I also (p. 178) take the ground that the zodiacal light 'is made of the streams of particles flowing away from the Sun, under the operation of a force of solar repulsion; or, in other words, is the indefinite continuation of the corona observed in total eclipses of the Sun, with its attendant streamers.'

"Without specifying farther what I have urged in support of this doctrine of visible material emanating from the Sun, I will refer you to the statements in the article on the corona which I send you by the same mail with this letter. I also send my paper 'On the Periodical Variations of the Declination and Directive Force of the Magnetic Needle,' published in the *American Journal of Science* (March and July, 1855), in which you will see (p. 47, &c.,) that I undertook to explain the irregu-

lar perturbations of the magnetic needle by electric currents developed in the upper atmosphere (or 'photosphere') of the Earth by the arrival of the solar matter, and took the ground that 'the matter radiated from the Sun, and falling upon the Earth's atmosphere, was probably the substance of terrestrial auroras.'

"I send you, also, the principal portion of my paper on Donati's comet, published in the *Journal of Science* (July, 1861), by which you will perceive, if I mistake not, that my investigations with reference to comets are at least worthy of recognition. In the second edition of my *Astronomy* (1845) I advocated, at considerable length, the theory of the development of the tail of a comet by the operation of a repulsive force exerted by the Sun, giving the credit of the origination of this theory to Olbers. On looking over the article on Donati's comet you will see that I have subjected the theory to the rigorous test of comparison with observation, and that I have shown, by careful computations, first, That the great cause of the wide lateral dispersion of the matter of the tail was an inequality in the Sun's force of action on different cometary particles; secondly, that I have assigned the actual limits between which the force varied; thirdly, that I have made out that not far from one-half the tail in breadth, on the concave side, was made up of particles that *were not effectively repelled by the Sun*, but separated from the nucleus, after having become disengaged from its influence, only because they gravitated toward the Sun with less force than the nucleus did,—a result certainly of great importance, and which, so far as I know, has not been to this day reached by any other investigator; fourthly, that all the particles variously acted on by the Sun, which left the head of the comet simultaneously, were, after the lapse of one, two, three, and five days, for a continued interval of some three weeks, distributed along successive *right lines diverging from the head* (see fig. on p. 61.) This result furnishes an explanation of the 'columnar structure of the tail of the comet' noticed by Bond and other observers, showing that it must have resulted from variations, from day to day, in the quantity of cometary matter ejected from the head. It also gives us an insight into the phenomenon of 'secondary tails;' they are to be regarded merely as lines of matter *variously* repelled, but, for the most part, by a much more energetic force than the maximum repulsion that prevailed along the preceding side of the principal tail. The results of the observations do not, however, require us to suppose that the particles at the extremity of the secondary tail (which were those most energetically repelled) had a higher average velocity than about one hundred miles per second; for the observed positions of the secondary tails show that they were made up of matter which left the head not less than three or four days before the date of observation (Oct. 5).

"I ought here to take occasion to explain an apparent error

in my calculations. On page 65 it is stated that the actual position of the nucleus was $18^{\circ}6'$ in advance of the calculated position. Soon after the publication of my article, I received from Prof. Bond a letter bearing date July 10, 1861, of which the following is an extract: 'I have just received the *American Journal* for July, and, in looking over your interesting article on the comet of Donati, see at once that in the chart on p. 64 you have taken 7^{h} Gr. for the time to which it corresponds, instead of $8^{\text{h}} 30^{\text{m}}$ Gr., for which it was actually projected.'

"The error was, I have no doubt, an inadvertence on my own part in the copy sent you. The correction will be easily made, as the tail moved, through nearly its entire length, on that day with a daily motion of $4\frac{1}{10}^{\circ}$ normal to its curve. The nucleus itself 4° . Making the correction referred to the calculated and actual positions of the nucleus differ only $3\frac{1}{2}'$."

Statement of Views respecting the Sidereal Universe. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

It appears to me desirable to conclude my discussion of the relations presented by the Sidereal Universe—so far, at least, as these pages are concerned—by indicating briefly but distinctly the points in which my views differ from the views advanced by those who have preceded me in the discussion of this subject.

I must premise that Sir John Herschel, the late Earl of Rosse, Whewell, and Herbert Spencer have advocated more or less decidedly some of the opinions which I have been led independently to enunciate. Regarded as a whole, however, my views are original, and several of the points to which I draw attention were first noted, I believe, by myself. If not, I would desire to be corrected, so as not to present as new views opinions which have already been advocated by others.

In the first place, then, I lay down as a fundamental rule that no *hypothesis* as to star-magnitudes or star-distribution can safely be adopted as a basis of research. In ordinary subjects of inquiry, it is well to have working hypotheses, varying perhaps as we proceed, but serving conveniently for the co-ordination of observations. But in researches into the constitution of the Stellar Universe, we must not adopt any hypothesis until observations sufficiently numerous and extensive have indicated its justice.*

* It is rather singular that my inquiries, *the first systematic inquiries ever based on observations as they stand, without assumption of any sort*, should have been so misunderstood by several as to be described as theoretical by comparison with such inquiries as the earlier work of Sir W. Herschel, based on assumptions

It was owing to his failure to recognise this principle at the beginning of his work (though, I need hardly say, it is to his work that I owe the main evidence in favour of the principle) that Sir W. Herschel formed the stratum theory of the sidereal system, which French and English text-books of astronomy persist in describing as a demonstrated theory, though Herschel himself definitively abandoned it. It will be well to consider this case fully, partly because no opportunity must be lost to indicate the mistaken nature of the text-book accounts of Herschel's work, and partly because of the intrinsic importance of the discovery which Herschel really effected.

It is known that, in 1784, Sir W. Herschel, who then supposed that our sidereal system is continuous within its limits, and consists of stars strewn (numerically, not in respect of real magnitude) with a certain general uniformity throughout the scheme, suggested his plan of star-gauging. It is clear that, adopting these hypotheses, a telescope powerful enough to penetrate to the limits of the system in all directions, could be employed to determine the shape of the system. For the number of stars in any given direction (within a given field) would afford a measure of the extension of the system in that direction.

And manifestly, if we add to the two hypotheses just stated this further hypothesis—that the sidereal system has some tolerably regular figure, as spheroidal, disk-like (circular or oval), cloven discoid, or the like, then a considerable number of scattered gaugings would afford sufficiently accurate general evidence as to the true nature of such figure.

It was from the combination of these hypotheses, and from a moderate number of gaugings, that, in 1785, Sir W. Herschel derived the impression that the sidereal system is shaped like a cloven flat disc, having its greatest extension towards the Milky Way, all whose stars form part and parcel of the system, and conform (according to the views of 1785) to the hypotheses adopted by Herschel.

In 1802, Herschel definitely indicated his abandonment of the hypothesis of generally equable distribution, and necessarily with that hypothesis he gave up the cloven-stratum theory of the sidereal universe, and the supposition that the stars of the Milky Way are distributed like those immediately surrounding us. The following passages from the paper of 1802 will serve to indicate Herschel's change of views even to those not well acquainted with his earlier papers; but the real significance of these passages will only be appreciated by those who have thoroughly mastered his whole series of papers (*which, judging from my own experience, implies the careful perusal of the whole series at least*

which were not only questionable in themselves, but were rejected eventually by Herschel himself, or with such investigations as those of Struve, based, as Encke long since pointed out, on assumption upon assumption! Those who are acquainted with the literature of the subject have, however, avoided this mistake.

three successive times, and the study of certain portions in the same way in which one would study some difficult mathematical proposition):—"The stars we consider as insulated," he writes, in 1802, "are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions to hold the intermediate stars in a state of rest. For though our Sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am now convinced, by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us. . . . On a very slight examination it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. . . . The milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near the undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way. . . . We may indeed partly ascribe the increase, both of brightness and compression" (in these clustering regions), "to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, *the space containing the clustering stars MUST TEND TO A SPHERICAL FORM, if the gradual increase of the stars is to be explained by the situation of the stars.*"

Nine years of labour passed before Herschel again dealt with general considerations respecting the sidereal universe.* I will quote one passage from the paper of 1811, which deserves to be carefully studied by some who have considered it a sort of high treason in me to question the soundness of the cloven-stratum theory of the sidereal universe:—"I must freely confess," he says, "that by continuing my sweeps of the heavens, my opinions of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and indeed

* Too commonly those who write of his views forget these long intervals of labour. We have now followed Herschel to the year 1811, *twenty-six years* from the time when the cloven-stratum theory was enunciated; yet I have seen sentences from the paper of 1811 quoted in immediate connexion with remarks from the papers of 1784 and 1785. Even at our own meetings I have heard assertions as to Herschel's views based on the combination of opinions in the papers of 1817 and 1818 with those expressed a third of a century earlier; while Struve, who advocated the later opinions of Herschel as against the views of 1784 and 1785, has been quoted in terms implying perfect agreement between his views and those expressed in the whole series of Herschel's papers. This is as though Laplace should be quoted as holding opinions identical with those which Kepler entertained when he broached to Galileo his ideas respecting the relations of the planetary distances to the dimensions of the regular solids.

when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted should, on examination, prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but *when we examine the Milky Way*, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have supposed nebulae to be no other than clusters of stars disguised by their very great distance; but a larger experience and a better acquaintance with the nature of nebulae will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed."

Another hypothesis adopted by Herschel, though it was not eventually abandoned like those on which his star-gauging was based, equally illustrates how unsafe is the adoption of hypotheses in inquiries directed to the subject of the constitution of the universe. I refer to that method of gauging which he based on the supposed space-penetrating powers of his telescopes. This method was suggested in 1817, when Herschel was in his seventy-eighth year, and some results of its application were discussed in 1818. It appears to me that, while the papers of 1817 and 1818 afford unquestionable evidence of *power*, they show equally clear traces of the failing elasticity of his wonderful mind. Having devised a working hypothesis, he works straight on with a diligence and effect worthy of his best days; but he does not look around for tests as he did in the days when the first method of star-gauging was in use by him. Or rather (for, in his earlier years, he did not so much search for tests as recognise those which were in effect applied as he worked), we do not find him quick to notice, in 1817 and 1818, how his results negatived his hypothesis.

I conceive that no one who considers the real significance of Herschel's results, in 1817 and 1818, can fail to admit that they are utterly inadmissible, and that they therefore show the theory on which they are based to be altogether untenable. Herschel had taken resolvability as a test of distance. But he found regions which, while partially resolvable with his lowest powers, were not wholly resolvable with his highest. These limited regions of the heavens indicated, according to his hypothesis, long stellar projections turned directly towards the Sun, appreciably cylindrical but really conical, with the Sun at their vertex — *a monstrous supposition, and one which Sir W. Herschel would certainly have rejected in his earlier years*. It was by reasoning justly on precisely such results that Sir John Herschel was led to regard the Magellanic Clouds as roughly spherical. Sir W. Herschel had arrived, as we have seen, at a precisely similar conclusion respecting the figure of the clustering aggre-

gations of the Milky Way. Yet it was to some of these very aggregations that he now applied the new gauging test, with results thus irreconcilable with those he had before obtained. It was by applying this new method that he arrived at the conclusions on which Struve afterwards so strongly insisted, that parts of the sidereal system are absolutely unfathomable. Yet his former and sound principle of interpretation, and the principle which Sir John Herschel, Whewell, and Herbert Spencer applied to the Magellanic Clouds, show that where *Herschel thought he was penetrating to the extreme limits of the sidereal system, he was in reality only analysing more and more searchingly an aggregation in which many orders of stars were mixed up. What he failed to do was, not as he supposed to sound the Galaxy, but to recognise as separate stars the minutest orders of orbs included within such aggregations.*

Struve, strangely enough, supposed that he himself had either made no assumptions which could fairly be regarded as such, or else none but the safest assumptions. Forbes and Encke more justly weighed Struve's researches. His assumptions, indeed, were the most daring which perhaps have ever been applied to the subject we are upon. Encke enumerates five assumptions, all of them questionable. I shall note only two, both occurring in the process by which he constructed his famous *Section* of the sidereal system, about which some have written in terms which would be exaggerated if applied to the labours of the elder or the younger Herschel.* Having counted (or rather obtained directly from Weisse's numbered catalogue) the numbers of stars, down to the ninth magnitude, in the different hours of an equatorial zone *thirty degrees wide* (from $+15^{\circ}$ to -15°), he first assumes that the distribution there indicated may be regarded as true *for the Equator itself*; and then, secondly, he assumes that the stars towards each hour of the equator may be distributed over the sector formed by radial lines from the Sun to the extremities of that hour-arc, according to certain assumptions as to the relative distances of the various orders of star-magnitude. The combination of the twenty-four sectors constructed on these assumptions, which are not only not supported by evidence, but strongly opposed by all the evidence thus far obtained, forms Struve's equatorial star-disk. On this insecure evidence, and on a singular mistake as to the meaning of an expression employed by the elder Herschel,† Struve based his famous hypothesis of the extinction

* I wish it to be carefully noted that, throughout my inquiries into the constitution of the heavens. I have not said one word which has indicated other than a just appreciation of the value of the Herschels' labours.

† Herschel wrote, that "*when* he could not resolve the Milky Way it was because it is unfathomable," meaning clearly that certain parts of the Milky Way lay beyond the power of his strongest telescopes. Struve probably in making extracts in German, wrote "*wenn*" for "*when*;" at any rate, in the French of his *Etudes d'Astronomie Stellaire*, we find the word "*si*" for the English "*when*," and thus the whole meaning of the sentence is modified.

of light, his analysis of the proper motion of the Sun in magnitude, and other results equally interesting, and, in my judgment, equally unreliable.*

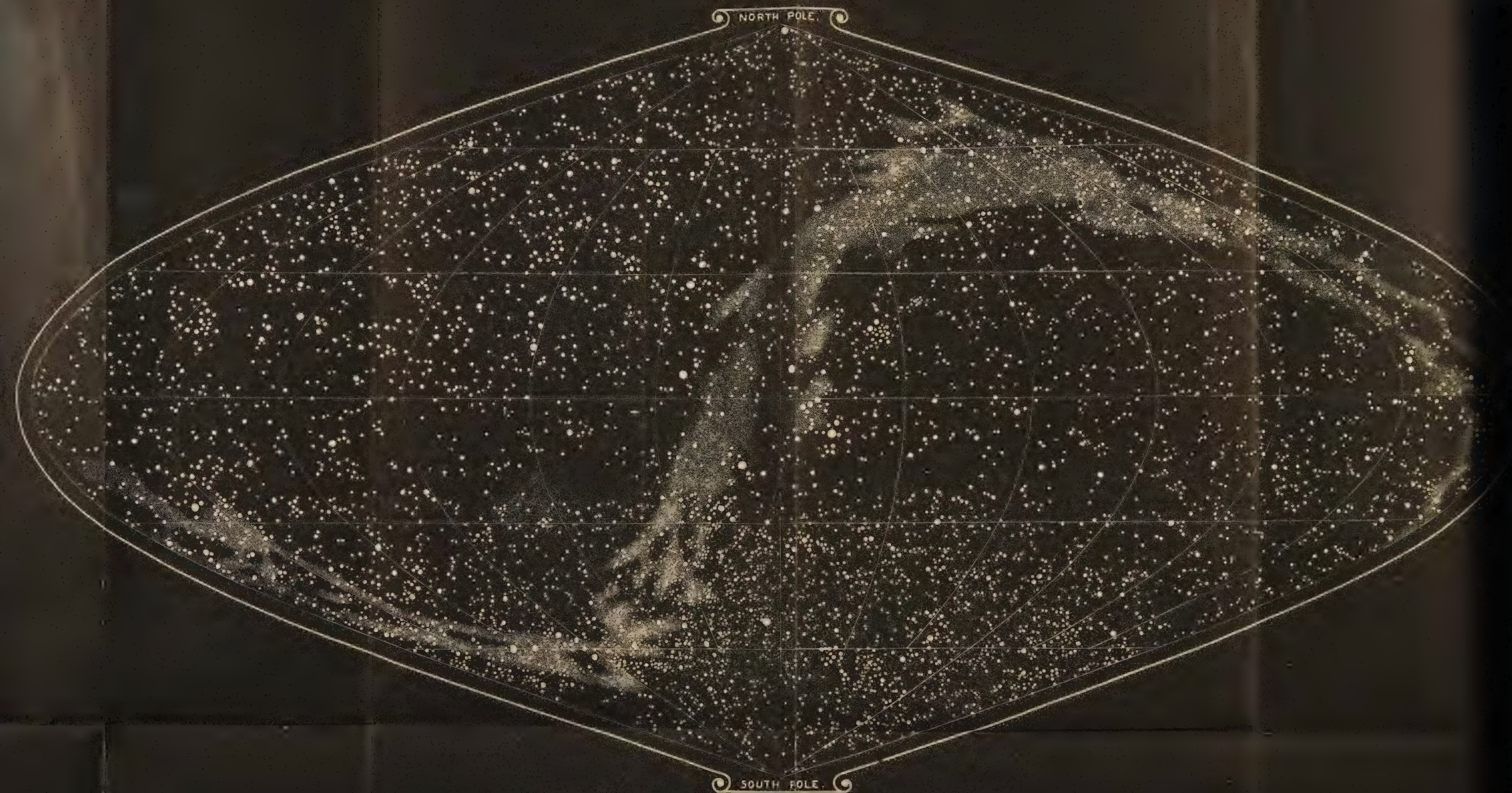
Sir John Herschel, so far as his own inquiries (as distinguished from those which he carried out in pursuance of his father's plans) were concerned, carefully avoided the adoption of any hypothesis, basing his opinions on observed facts solely. I must note, however, that he does not show the same acquaintance with his father's series of papers as Struve appears to have obtained. I think this is not altogether to be wondered at. Sir John Herschel took too large a part in original astronomical investigations to have leisure for that thorough investigation and analysis of his father's series of papers by which alone their real bearing can be ascertained. Be this as it may, the fact is patent that whereas Sir W. Herschel abandoned the method of gauging which he had adopted in 1784, Sir John Herschel applied this method to the Southern heavens. It is not wonderful that he obtained a result not according with that which his father had obtained in 1785, for it is the property of erroneous methods to lead to incongruous results. It was thus that the cloven flat-ring theory was adopted by Sir John Herschel in preference to the cloven flat-disk theory.

But Sir J. Herschel's discussion of the peculiarities which he had recognised in the distribution of stars, his analysis of the features of the Milky Way, and his treatment of the evidence derived from the Magellanic Clouds, are characterised by sound and clear-sighted reasoning. We owe to him, in fact, the initiation of those processes and methods of inquiry which have recently been applied in a more systematic manner.

Passing to my own treatment of the subject, I remark, first, that it has seemed to me very necessary to substitute for statistical research, at least in the beginning of inquiries into the laws of stellar distribution, some process not requiring preliminary hypotheses. I can conceive no general statistical process absolutely free from hypothetical considerations. Statistics can be satisfactorily applied to inquiries suggested by other and less deceptive processes; but at the beginning we cannot *count*, except on accordance with some pre-arranged plan, and such plan must necessarily be based on hypothesis. We see this in Struve's researches. He counted the number of stars in given hours of right ascension; but the result was meaningless, except on the assumption that the distribution of stars over a given hour, and between declination-circles $+15^{\circ}$ and -15° , possessed a certain significance. We see, in fact, that he regarded the richness of

* I write this with some confidence. Sir J. Herschel has shown the weakness of the reasoning on which Struve based the theory of the extinction of light. As to the rate of the Sun's proper motion, I showed long ago the probability that this rate exceeds that assigned to it by Struve; and Dr. Huggins's recent researches into stellar motions of recess and approach may be regarded as conclusive on the subject.

represented, the great purpose of star-mapping, so far as concerns our subject, is fully served. However, it would be a useful exercise to those having sufficient leisure, to make other similar



THE STAR-SPHERE ON FLAMSTEED'S PROJECTION (EQUAL SURFACE) SHEWING ALL THE STARS VISIBLE TO THE NAKED EYE.

distribution around the Equator itself as indicated by the richness of star-distribution over areas thirty degrees in declination by fifteen degrees in right ascension ; and unless that assumption, or some like assumption, were made, the mere numbers he obtained could not, so far as I can see, have any significance at all.

But then there is the risk in such a case that we may be misled in giving significance to statistical results by means of some hypothesis,—the result only becoming significant by being made erroneous.

Charting, so long as it is isographic, seems open to no such objection. We plot down stars, or nebulae, or variables, or red stars, or double stars, as the case may be, and accept the result, whatever it may be, certain at any rate that we shall not be taught anything erroneous, though the charting may not lead to any result of importance. Whatever there is to learn, we shall probably learn from a well-constructed chart ; and we may learn much more than had been hoped when the chart was commenced, or else—and this has repeatedly been my own experience—a chart, while not in itself teaching any noteworthy lesson, will suggest either statistical research or some new process of charting by which important new knowledge may be obtained.

From my equal-surface charts of the brighter orders of stars, I infer—I think I may even say I have demonstrated—that there is a much greater range of difference in real star magnitudes than had been supposed. Unless the laws of probability are to be questioned, those lucid stars which gather richly over the area of the Milky Way are really immersed among the clustering aggregations of smaller stars which produce the milky light of the Galaxy. In this case those brighter stars must be not only *much* larger, but many hundreds of times larger than the fainter stars of the Galaxy.

We have here the first step towards just views of the constitution of the Milky Way, or rather the next step beyond the great, but little noticed, discovery of Sir W. Herschel's, that the bright clouds of the Milky Way are for the most part spherical clusters of stars. The excess in the number of lucid stars in the Milky Way proves that the stars of our constellations are among the leading orbs of these spherical aggregations. The accompanying charts, viz. the chart of stars and nebulae, and the wood-engraving, serve to exhibit other peculiarities illustrated by the mapping of stars visible to the naked eye. Little is required by way of explanation of any of the charts, which are, as all such charts should be, self-explanatory. It is only necessary to mention that the large wood-engraving exhibits the whole heavens on Flamsteed's projection (which is an equal-surface one) ; and although in parts the distortion is monstrous, yet as the distribution is always truly represented, the great purpose of star-mapping, so far as concerns our subject, is fully served. However, it would be a useful exercise to those having sufficient leisure, to make other similar

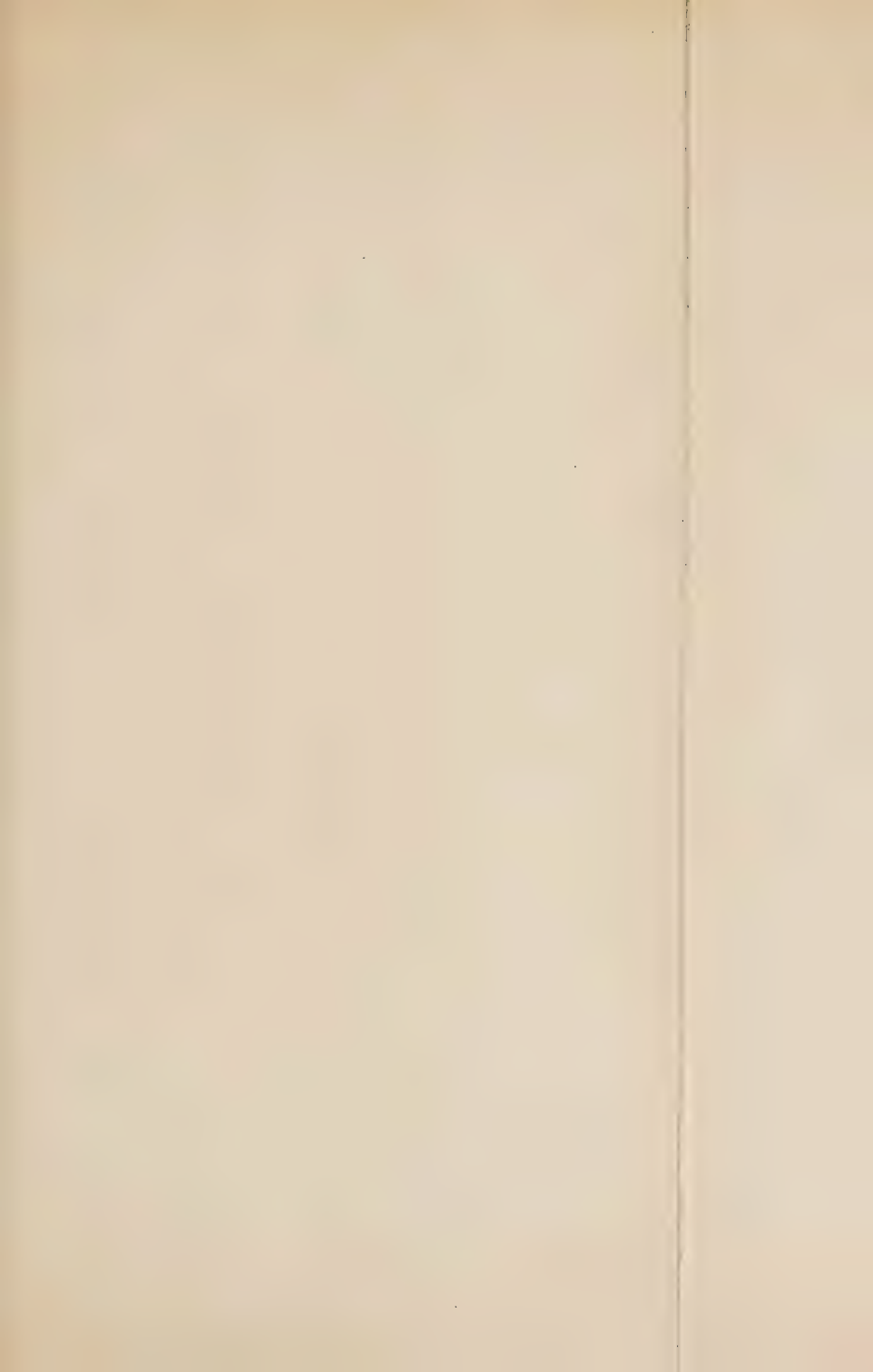
charts on the same projection, but taking other meridians for the central line. Charts on the same projection, but on a larger scale, and including the nebulae, as in the lithographic engraving, would also be highly instructive. I can, however, only indicate at present the desirability of such charts, my time being too fully occupied with other matters to enable me to carry out all the mapping processes which I should like to see applied. Before long, should I not be anticipated, while carrying out the processes of star-gauging which I have already commenced, I shall probably also obtain opportunities for constructing a complete series of charts illustrative of the laws of stellar and nebular distribution; but for the present those already drawn may be regarded as sufficient. Indeed it is not to be expected that others *applied to the same objects*, would reveal new relations of importance, though strengthening and elucidating the evidence given by those already drawn.

Charts including stars down to the seventh and eighth orders, yet remain to be constructed. I am not acquainted with any suitable materials. For the purpose of true star-gauging it is essential that the same uniform scale of star-magnitudes should be used; and as yet no complete catalogue or series of charts in which the stars of the seventh and eighth magnitudes have been estimated according to a uniform scale, are accessible, or at least known to me. I may remark, indeed, that charts alone would serve my purpose, since the labour of constructing equal-surface maps from complete catalogues, including stars down to the eighth magnitude, would be enormous, and though the results would be interesting, they would probably scarcely repay the labour.

The existence of Argelander's series of charts of stars visible in the Northern heavens with a telescope $2\frac{3}{4}$ inches in aperture alone rendered my next step—the construction of an equal-surface chart containing all such stars—a feasible, though not altogether an easy one. I must remark, that the charts of Argelander's series cannot be regarded as quite exactly representing the distribution of stars over the whole of the Northern heavens. Anyone who will test them systematically with the telescope, will soon recognise the fact that many stars well within the range of a telescope $2\frac{3}{4}$ inches in aperture have been omitted. This, of course, was to be expected. But it is remarkable that in many cases stars have been noted which are certainly not ordinarily to be seen with Argelander's selected small telescopic power.

Nevertheless, regarded as a whole, the series of charts constructed under Argelander's superintendence represents fairly the distribution of stars down to the eleventh magnitude of Sir J. Herschel's scale. The inexactnesses relate to minutiae, not to the broad features which can alone be dealt with in the present stage of the inquiry.

I must remark here, that although in my equal-surface chart





SECTION FROM A CHART OF 324 198 STARS ON AN EQUAL SURFACE PROJECTION.

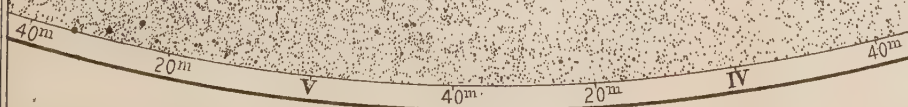
A. Proctor del

M&N Harshart photo lch

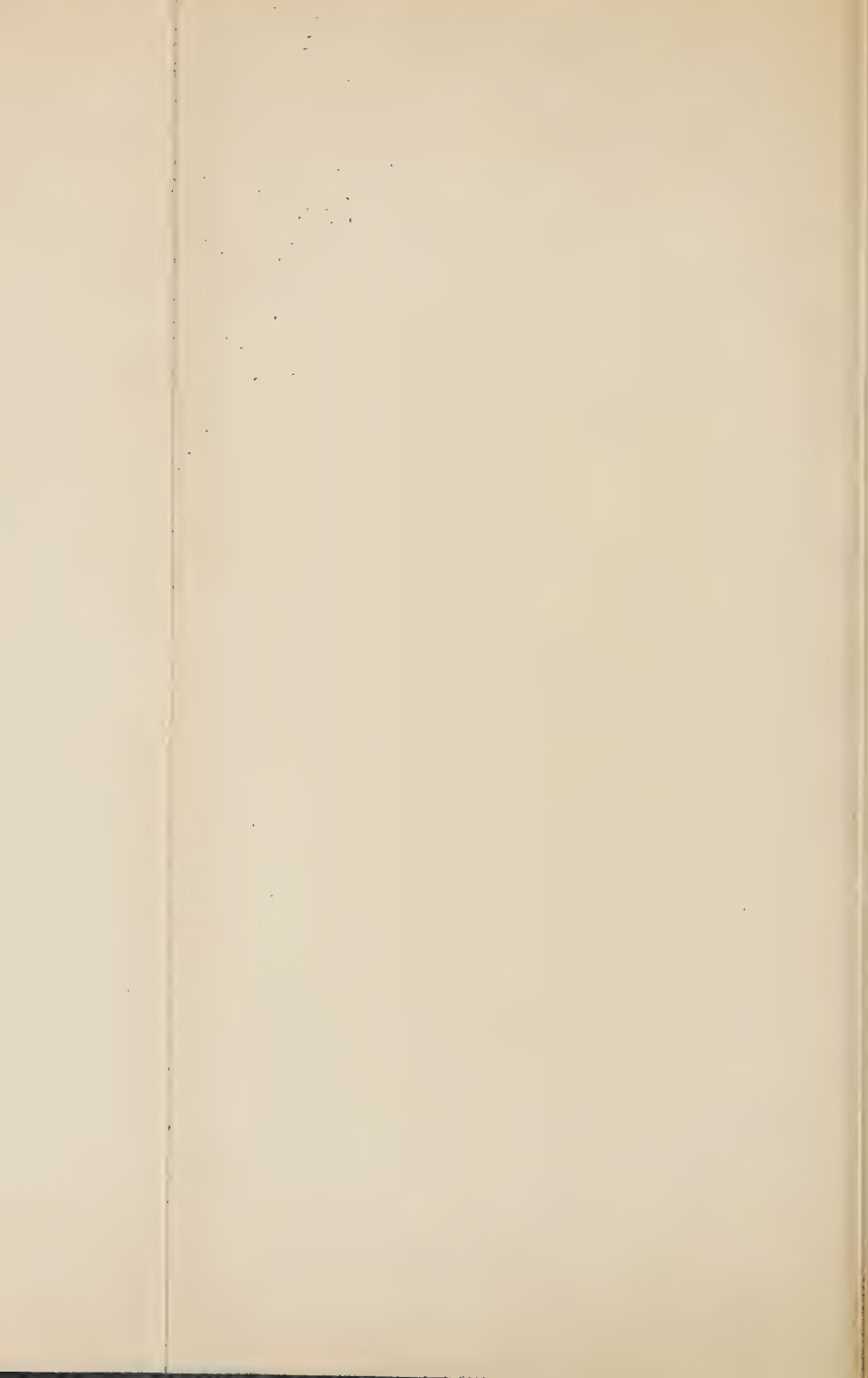
CAMELOPARDUS.

AURICA.

ORION

PERSEUS α CASSIOPEIATAJAJUS *Puadoe*

SECTION FROM A CHART OF 324,198 STARS ON AN EQUAL SURFACE PROJECTION



including Argelander's series, I have marked down all the stars, as nearly as the eye could judge, in their true place within the spaces (one degree each way in R.A. and Decl.), to which they severally belong, I have by no means done this with the intention of constructing a chart which could be used for reference to individual stars. In point of fact, I wanted only to ascertain the general distribution of the stars, and my purpose would have been equally, or nearly, as well fulfilled if in each space I had marked down *at random* the proper *number* of stars. But, strange though it may seem at a first view, it was easier for me to make an exact transcript for each space than merely to jot down so many stars. The reason is, that by copying from each space I had at once a means of distinguishing one space from another in my chart as compared with the original. Nothing could have been more baffling than the attempt to preserve merely the numerical distribution; and of course the result was somewhat more accurate, even as regards the indication of laws of aggregation.

The significance of the results obtained by this second step—that is, the step from stars visible to the naked eye, to stars visible with $2\frac{3}{4}$ inches of aperture—can only be appreciated by those who have carefully followed, and thoroughly mastered, the reasoning of the elder and younger Herschel, and of the elder Struve. I assert, without the slightest fear of contradiction by any possessing such knowledge, that *the broad teaching of the equal-surface chart of 324,000 stars disposes finally of all theories of the constitution of the sidereal universe which had previously been enunciated*. The chart does not definitively indicate a new theory—rather it suggests the idea that the constitution of the sidereal universe is too complex to be at present ascertained. But it completely negatives (i), the stratum theory (even in the modified form apparently retained by Sir W. Herschel); (ii), the flat-ring theory of Sir John Herschel; and (iii) the infinitely extended stratum theory, with condensation towards the mean plane, which Struve adopted.

The force of the evidence derived from the chart (specimens from which are herewith submitted) consists in the position which the gauging power of Argelander (for it is to his gauging power that I alone appeal) bears to naked eye vision on the one hand, and to the powers used by the elder Herschel on the other. Powers intermediate to Argelander's and Herschel's may give important evidence, and doubtless will, when applied in accordance with the plan I have elsewhere suggested; but such evidence cannot have the value which resides in Argelander's work. A much lower telescopic power than Argelander's would bring the range too near that of ordinary vision; a much higher power would bring the range too near that of the Herschelian gauging telescopes. It is the intermediate character of Argelander's telescopic survey which constitutes its real value.

This I propose briefly to indicate.

The eye shows the great Milky Way stream, with its irregularities, branching extensions, nodules, and other peculiarities. The eye also indicates (in combination with suitable processes of charting and enumeration) the gathering of lucid stars on the Milky Way. Such is the evidence at one extremity of the scale. At the other we have the result of the Herschelian star-gauges, and especially of those gauges, not tabulated, which both the Herschels applied to rich Milky Way regions; and we learn from it, according to the words I have already quoted, that in Herschel's sweeps of the heavens "it has been fully ascertained that the brightness of the Milky Way arises only from stars, and that their compression increases according to the brightness of the Milky Way." This relates, as we know, to the stars brought into view by his great telescopes. Nothing, then, can be more interesting and important in connexion with the subject of the sidereal universe than the question whether the naked-eye brightness, and the gathering of naked-eye stars on the one hand, and the great richness of stars of the faintest orders reached by our telescopes on the other, are accompanied by an exceptional richness of stars brought into view by an intermediate power,—far enough removed from naked-eye vision on the one hand to give those large numbers which raise probabilities into certainties, and far enough removed from Herschel's space-penetrating power on the other hand to ensure a real selection of larger stars from among the practically infinite numbers of minute stars revealed to his scrutiny. This would in any case be important, as Struve long since demonstrated, and as Herschel himself clearly recognised, as affording general evidence of the laws of stellar condensation; but the comparison assumes a far greater importance when we remember what Herschel demonstrated as to the generally insulated nature of those aggregations which lie along the Milky Way streams. Let the meaning of his result be rightly appreciated; let it be clearly remembered *that he had in effect proved the Milky Way to consist in the main of real clouds of stars arranged in real streams in space*, and we see at once that to prove that these clouds are richer, both in minute stars and in relatively bright stars, than surrounding regions, is to prove the existence, within each of them, of widely different orders of real star magnitude. When we add the consideration that the Herschels, with their most powerful telescopes, were in some cases unable to resolve the richer regions of the Milky Way, we begin to recognise something of the complexity and variety of structure existing where uniformity of structure and variety of distance had been imagined.

It is obvious that what has thus been recognised respecting the system of stars is in perfect accordance with what has been ascertained respecting the distribution and nature of the nebulae. So long as we assumed the stellar system to be of a tolerably uniform nature throughout its extent, there was some difficulty in admitting as probable the existence within that

system of structures so distinct as the nebulae, whether resolvable, irresolvable, or gaseous. Even the evidence afforded by the Magellanic Clouds as to the nature of all the orders of nebulae, convincing though that evidence really is, did not impress the truth on the minds of those who yet could point to no flaw in the reasoning. The relations indicated by my equal-surface charts of the nebulae, though equally satisfactory as evidence (besides being independent and therefore the more convincing), were similarly inoperative; and I think the beautiful, and in my judgment most valuable maps by Mr. Waters which appear in the present number, would not carry conviction of themselves.

But I submit that when we perceive the variety of stellar structure and aggregation, as well within the Milky Way as in extra-galactic regions, the evidence respecting nebulae is brought into such perfect accordance with the evidence respecting stars that its real significance can no longer be misapprehended.

Here, moreover, the common evidence given by stars and nebulae (agreeing also perfectly with that given by the Magellanic Clouds) is strengthened by the evidence derived from *details* of structure in the Milky Way, on its borders, and outside of it. I cannot here do more than allude to the nature of this evidence. I would note, then, that the balance of attractions within the Milky Way which Sir W. Herschel pointed out, suggests a reason as well for the paucity of irresolvable nebulae within its bounds, as for the number of irregular clusterings and ordinary clusters contained within it. As Sir W. Herschel pointed out, the clusters in and near the Milky Way may be looked upon as so many portions of the great mass drawn together by the action of a clustering power of which they tend to prove the existence. But this clustering power has been interfered with and checked by the balance of attractions. Outside the bounds of the Milky Way, a clustering power uncontrolled by any such cause has been at work, and to its influence may be ascribed those closely set nebular clusters which are either irresolvable or can else only be resolved with very powerful telescopes.

The gradual diminution in the number of clusters as we leave the galactic zone and the equally gradual increase in the number of irresolvable nebulae show the reality of the connexion here indicated. There is an even more convincing though more delicate proof, in the continuous change of character of the nebulae from the most scattered clusters of the Milky Way to the absolutely irresolvable nebulae of extra-galactic regions. Of course the actual constitution throughout the whole heavens has in part depended on the distribution of star material, and not on position only. We see the influence of this in the charts of stars and nebulae in *Virgo* and *Coma* which I contributed some time since to our Proceedings, and on a wider scale in Mr. Waters' beautiful charts, where we notice how, as irresolvable nebulae become scarce, scattered clusters appear, not only on the heavens regarded as a

whole, but in the several parts of the extra-galactic nebular system, and streams.

The evidence respecting nebulae known to be gaseous points the same way ; but it may be desirable to wait before summing up this part of the evidence until the spectroscope has been more widely and systematically applied to the nebulae.

It is, perhaps, hardly necessary to insist on the evidence which the discussion of stellar proper motions can afford on questions relating to the structure of the universe. The general tendency of star motions, and the average magnitude of such motions would supply evidence of extreme importance towards the discrimination of stellar aggregations in particular regions of space.

And here I must point out two mistakes which have been made by some persons who have spoken of the phenomenon which I have called "star-drift."

One mistake is the confounding of this phenomenon with the star-drift due to the Sun's motion in space, which drift was first recognised by Sir W. Herschel and has since been more widely recognised by several other astronomers. This star-drift is general, but is seen only in the average of stellar motions. The star-drift which I have indicated is local, and has a different significance altogether. Before I had recognised it (in fact, before I had contributed any paper to the pages of our *Notices*), I suggested its probable existence. When I had recognised it in several regions of the heavens, as in *Gemini*, *Ursa Major*, and so on, I pointed out how my theory on the point could be tested by the spectroscope, and I selected the drift in *Ursa Major* as specially suitable for the purpose. I described the results which should follow if my theory were sound, and as is now well known my expectations were confirmed to the letter. The star-drift thus demonstrated in *Ursa Major* is a phenomenon perfectly distinct from the star-drift due to the Sun's motion. It takes place, as my maps of the stellar proper motions show, in almost exactly the reverse direction from that due to the Sun's motion. Moreover the real interest of the phenomenon consists, *not* in any evidence it gives as to the general motions of the sidereal universe, but in the fact that it marks out the stars affected by it as forming a subordinate system separated from other star-regions by starless spaces of enormous extent. Moreover the recognition of star-drift, especially if accompanied by spectroscopic evidence as to star structure, will probably become a potent means of ascertaining the architecture of various parts of the stellar universe.

The other mistake resides in the supposition that at the present stage of the inquiry it would be desirable for me to examine and discuss those observations which promise to afford more complete information than we now possess respecting the proper motions taking place within the star system. I can only deal with wide and general relations ; and the charts of proper motions already constructed by me suffice for that purpose, since their construction

has led to the general theory of star-drift which it was chiefly important to establish. Anyone who may have leisure for the work can now examine suitable star-catalogues for the recognition or correction of estimated proper motions, with the assurance that such work will reap a rich reward. But for my own part the task I have taken in hand is of another nature, and will permit of no expenditure of time on details, however interesting the subsidiary evidence which such details may afford. I wish rather to set others at work on the various parts of the wide subject of the constitution of the heavens than to enter myself on work of detail. The structure of the star-system as a whole, or at least of the portion within our scope, is the subject to which I address myself, and too minute a discussion of the subordinate details would impair rather than improve the general view which I desire to obtain. The proportions of a great structure are not favourably studied by approaching as near as possible to some particular part of it.

Here my discussion of the subject of the constitution of the stellar heavens comes to a close so far as these pages are concerned. I take the opportunity of thanking the Fellows of the Society for the patience and attention with which I have been listened to when I have described my views at our evening meetings. I trust and believe that those who have really examined the evidence I have adduced, will not have thought me dogmatic in the expression of opinions which I have based on a very thorough investigation of all the available evidence. I have been more than satisfied with the support I have already received. From Sir John Herschel in particular, who perhaps alone of all our leading astronomers has taken special interest in the subject of the constitution of the heavens, I received encouraging expressions of opinion (which will one day be published), important not only in their relation to the researches I communicated to him, but in their prospective bearing on researches which have been completed since his lamented death. For the rest, it is not to be expected, and is in fact not possible, that general attention should be drawn to a subject so far removed from ordinary astronomical studies, whether practical, observational, mathematical, or theoretical. At all times in the history of Astronomy, the number of those who have been attracted to the study of the laws of stellar distribution has been small, though strictly speaking the very name Astronomy implies such study more specially than any other now recognised subject of astronomical research. But the true student of science is content if he "fit audience find though few." The investigation and demonstration of the truth are more important than the mere numerical array of supporters: (though, indeed, if on the one hand, as I had expected, few have been attracted to a subject of great difficulty, yet on the other I have had small occasion to complain of cavil or objection). And if the results to which I have been led, or may be led hereafter, are just,

it is a matter of indifference whether they be generally accepted at a late or early season, in my own lifetime or not till long afterwards.

The Rotation-Period of Mars. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

In the *Annalen der Sternwarte in Leiden*, for 1872, there is a series of papers on the planet *Mars* by the late Prof. Kaiser. One of the papers relates to the rotation-period of the planet, and Prof. Kaiser therein endeavours to show that his estimate of the rotation-period of *Mars* — viz. $24^h 37^m 22^s.62$ — is preferable to mine — viz. $24^h 37^m 22^s.735$. When I first read this paper, it appeared to me that, whether Kaiser were mistaken or not in his conclusions, a great difficulty was introduced; because he stated as the result of his calculations, that a drawing of *Mars* by Huyghens, in 1672, cannot be reconciled with the two drawings by Hooke, in 1666, on which I placed reliance, unless a mistake of two hours in the time be accounted for. Even then,—that is, after my first cursory reading of his paper,—it seemed to me that the readiest solution of the difficulty was not, as Kaiser suggested, to reject the work of one or other observer, but to divide the error between them. Yet it seemed clear to me that this would involve the supposition of very careless drawing *both* by Huyghens and by Hooke. I have been glad, however, to find, when my leisure permitted me thoroughly to examine Kaiser's work, that the greater part of this discrepancy is accounted for by a rather strange error in his reckoning. In estimating intervals between the observations in the seventeenth and nineteenth centuries he would seem to have forgotten that the years 1700 and 1800 were not leap years, and so makes the intervals systematically two days too great. As he thus gets two Martian rotations too many, and each exceeds a terrestrial day by $37^m 22^s.7$, the error amounts approximately to the loss of $1^h 14^m 45^s.4$. This reduces the discrepancy by more than one-half. Oddly enough, a second error in calendar matters has prevented Kaiser from detecting the error, as he might have been expected to do when he compared the observations of Hooke and Huyghens. For Hooke's observation, made on March 3, Old Style, he translates into March 14, New Style, whereas the real reading is March 13. Thus an error of $37\frac{1}{2}$ minutes is introduced as between Hooke's observations and Huyghens', and this raises the discrepancy between them so nearly to that which *should* exist if Kaiser's rotation-periods were correct, as to satisfy the requirements of the case.

As regards the balance of difference between the two esti-

mates, I believe what I shall now proceed to say can scarcely fail to be regarded as a fair view of the evidence on both sides; and inasmuch as it gives (for the first time) an account of the whole evidence, so far as the most ancient observations of *Mars* are concerned, it must be held as for the present determining the most probable value of the planet's rotation-period.

Let it first be noted that the two estimates lie very near each other. A difference of $0^s.115$ or $\frac{2}{200}$ ths of a second, cannot be regarded as serious; and when correction is made for Kaiser's error as to the years 1700 and 1800, the difference is greatly reduced. And here I may remark that I fully agree with Kaiser's opinion, that Linsser and others exaggerated the accuracy with which the rotation-period can be determined. I note that, although I give the seconds to the third decimal place, I only do this to indicate the most probable value. I mention that the value appears to me to lie between $22^s.73$ and $22^s.74$. Kaiser sets the value at $22^s.622$ with about equal confidence in the two last decimal figures. It seems to me to follow from the evidence cited in his latest paper, that he and I have ourselves erred slightly in the point we have both dwelt on as involving a mistake (though a larger one) on the part of others, and that at present we must be content to regard the probable error as somewhat larger than the value at which we had agreed in estimating it.

The question turns on the ancient observations employed; for, as Kaiser very justly remarks, when an old observation is compared with modern observations taken within a comparatively short interval of time, the resulting rotation-periods will be almost identical, even though the two modern observations be affected with a considerable error. I must confess, however, I cannot understand why this remark is expressed as a correction on my own statements respecting the two observations in 1864 and 1867, which I compared with Hooke's observations in 1666; seeing that I prefaced those remarks with a statement to precisely the same effect as Kaiser's ostensible correction.

The (corrected) difference between Kaiser's result and mine arises from our selection of different observations in the seventeenth century to start from. Kaiser depends on a drawing by Huyghens, fig. 1, bearing date, August 13, 1672 (New Style) at half-past ten, Paris time. I employed two drawings by Hooke, figs. 2 and 3, taken on March 12, 1666, at $12^h 20^m$ and $12^h 50^m$ Greenwich time (New Style, and astronomical hour).

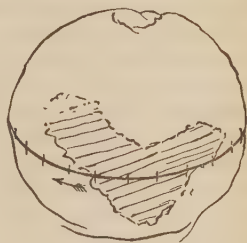


Fig. 1.

Taking Hooke's pictures for comparison with observations made in 1864, 1867, and 1869, I obtained the rotation-period $24^h 37^m 22^s.735$. Singularly enough, Kaiser, using his own observations in 1862, for comparison with Hooke's, obtains precisely the same period, even to the last decimal place, though

there is a triple discordance between his treatment of the matter and mine, for he sets the date of Hooke's observation one day too

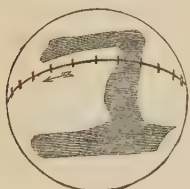


Fig. 2.



Fig. 3.

late, counts two days over by taking 1700 and 1800 as leap-years (I assume this is the way in which he gets his two extra days), and using his own observations in 1862, introduces a considerable time error, unless Beer and Mädler, Dawes, Browning, and others, have all made large time errors. Using an observation by Beer and Mädler, in 1830, he obtains, with Hooke's, the period $24^h 37^m 22^s \cdot 706$, or $\cdot 029^s$ less. He dwells on this as a strong point against my estimate, failing to notice that, in his own treatment of his own estimate, he deduces from the observations of 1862, combined with Huyghens', the period $24^h 37^m 22^s \cdot 643$, while he deduces from the observations of 1830, combined with Huyghens', the period $24^h 37^m 22^s \cdot 595$, a difference of $\cdot 048^s$. This, of course, was to be expected, from the singular medley of obvious errors appearing in a paper laboriously exact in more recondite matters.

Kaiser deduces from Huyghens' observations, combined with a great number of modern observations, the value $24^h 37^m 22^s \cdot 622$. In discussing Hooke's observations he introduces only one day in excess, because of the mistake he made in correcting Hooke's date for style. Huyghens' observations being dated in new style, there is no compensation of errors, and thus we must take into account the two extra days derived from the common years 1700 and 1800, treated by Kaiser as leap-years. Kaiser makes the interval between Nov. 1, 1862, $6^h 10^m \cdot 1$, and August 13, 1672, $12^h 10^m \cdot 3$, (at which epochs he finds that *Mars* was in the same position, as regards sidereal rotation), to be $69476^d 17^h 59^m \cdot 8$; and he proceeds, "In this period *Mars* made 67719 rotations; and the resulting rotation is $24^h 37^m 22^s \cdot 643$." In reality the interval is $69474^d 17^h 59^m \cdot 8$, and in this interval *Mars* made 67717 rotations; hence the calculated rotation-period is $24^h 37^m 22^s \cdot 71$; very near, it will be observed, to my estimate. However, as will be inferred from a former remark of mine, Kaiser's observations in 1862 do not accord with those made by several other observers. He himself notes this; and if we adopt his mean from various modern observations, we infer, after correction for the extra two days, the period $24^h 37^m 22^s \cdot 69$, by combining the observations of Huyghens with those made in modern times.

Kaiser was not aware of the existence of Hooke's observations when he used Huyghens'; and though I was aware of the existence of Huyghens' drawings (for Webb mentions them in his *Celestial Cycle*), I had not seen them when I used Hooke's.

Commenting on Kaiser's value, I remarked that it would throw the planet out at the period of Hooke's observations by an amount corresponding to the rotation during $2^h 20^m$. This would correspond to a rotation angle of about 34° , and would set the features shown in figs. 2 and 3, towards the *right*, making the north-and-south sea fall very near the edge. "This," I wrote, "is not to be thought of for a moment."

On the other hand, it appeared to Kaiser, in consequence of the mistakes already noted, that my period would throw the planet out at the period of Huyghens' observations by about the same amount, the great sea shown there being thrown by about 32° rotation towards the *left*. Kaiser reasons that *this* cannot be thought of; and that, therefore, choice must be made between Hooke's pictures and the drawing made by Huyghens. He conceives that there can be no question that the sea in Huyghens' picture is the sea which, for convenience of reference, I have called Dawes' Ocean; and that it is very doubtful whether Hooke's picture represents the same Ocean with Kaiser's Sea running north and south.* On the first point I fully agree with him. The feature depicted by Huyghens can be no other than the Dawes' Ocean; indeed, even if the supposed difference of two hours were assumed to operate, the Dawes' Ocean would still occupy the disk of this planet estimated for the hour of Huyghens' observations. But I can by no means agree with Kaiser that the feature depicted by Hooke is doubtful. It does not seem to have occurred to Kaiser to construct a picture of *Mars*, at the time of the observation in 1666, *using his own rotation-period*. If he had done this, he would have seen that the continent called Herschel I. would fall over the space where Hooke shows a well-marked north-and-south sea. Absolutely no feature on *Mars*, except the Kaiser Sea, *could* look like the north-and-south sea depicted by Hooke; but the region which would fall where this sea is shown, if Kaiser's period is correct, is the most unlike conceivable.

In fact, I think Kaiser would not have remarked on the uncertainty of Hooke's drawing if he had seen views of this planet at the same rotation-phase, and when in the part of its orbit where Hooke observed it. Hooke's aspect is quite as characteristic for the summer season of the Martial northern hemisphere as Huyghens' for the winter season of the same hemisphere. As

* "Hieraus würde folgen," he says, after stating the facts, "dass die Abbildungen von Hooke und Huyghens nicht denselben Fleck darstellen können. Es scheint mir nicht schwer zwischen beiden eine Wahl zu treffen. Huyghens's Darstellung ist eben so bestimmt und sicher als die von Hooke unbestimmt und unsicher ist. Hat Huyghens den Flecken f. (Dawes Ocean) wirklich gezeichnet und sich in der Zeitengabe nicht gänzlich geirrt, so muss Hooke's Abbildungen für eine Bestimmung der Rotationszeit unbrauchbar sein."

to the accuracy of the two drawings there is not much to choose perhaps. One cannot be enthusiastic over the artistic qualities of Huyghens' drawing, and the fact mentioned by Kaiser, that of twelve drawings by Huyghens, this is the only one in which any known feature of *Mars* can be recognised,* does not suggest either excellence of telescopic performance or surpassing skill in delineation. Hooke's drawings are in some respects much better; but it is easy to perceive from the aspect of other drawings by Hooke representing *Mars* as his distance increased, that the telescope employed was barely equal to show even the Kaiser Sea when *Mars* was close to opposition. This would introduce a difficulty in the delineation of the planet which telescopists and draftsmen will be familiar with. Accurate drawing becomes very difficult, even to skilful artists, when the features to be delineated are only to be seen at intervals, and then only (when definition is for the moment at its best) with exceeding faintness.

It seems to me that unless we absolutely reject the work either of Hooke or Huyghens, we must use both. If I had known of Huyghens' observations, I should certainly not have trusted implicitly to Hooke's; and on the other hand, I cannot agree with Kaiser in trusting implicitly to Huyghens'. It is strange that the idea should not have suggested itself to Kaiser, that though the whole difference of upwards of two hours' rotation cannot possibly be ascribed either to Huyghens' work or to Hooke's, it is perfectly possible to divide the error between Hooke and Huyghens.

Let us first consider to whose work the greater proportion of the error should be ascribed. I think we cannot prefer Hooke's work to Huyghens'; first, because Hooke gives two drawings closely according, and secondly, because the only part of Huyghens' drawing which includes the real outline of Martian oceans lies near the edge of the disk. The southern boundary of the Dawes' Ocean, as seen, is known to be variable, owing to clouds or other peculiarities of the Martian atmosphere. It is also always ill-defined; but the northern boundary is well marked. Now any feature near the edge shifts very slowly owing to rotation; and although two hours would certainly change the aspect shown in Huyghens' picture to one differing too much from it to be mistaken, yet one hour's rotation could fairly be ascribed to errors in his very rough drawing. On the other hand, it is not easy to suppose that *both* Hooke's drawings would be in error by so much as three-quarters of an hour's rotation, simply because a feature like the north-and-south sea changes in apparent position when approaching the centre of the disk more notably by far than a curved outline of an ocean like that shown in Huyghens' picture.

* Kaiser writes:—"Unter den 12 Abbildungen des Planeten Mars, welche Huygens in seinem Tagebuche mit der Schreibfeder eingetragen hat, giebt er nur eine einzige, in welcher sich einer der in den Jahren 1830, 1862, und 1864, beobachteten Flecken mit voller Gewissheit zurückfinden lässt."

Fortunately, when Kaiser's calendar mistakes have been taken into account, we have no occasion to attribute such large errors either to Huyghens or Hooke. I will give Kaiser's comparison between the two observations, and then the just comparison, so that not only the actual correction may be indicated, but the proportion which it bears to the correction Kaiser supposed to be necessary.

Regarding Hooke's observations as I have done (but with the object of showing I was mistaken), he finds the following epochs for the passage of the Kaiser Sea through the same part of a sidereal rotation :—

Huyghens	...	...	1672, August 13, $12^{\text{h}} 10^{\text{m}} 6$
Hooke	...	...	1666, March 14, $2^{\text{h}} 56^{\text{m}} 1$
Difference	...	...	2344 days, 9 $14^{\text{h}} 5$

In the interval *Mars* made an exact number of rotations ; *but* taking the period as $24^{\text{h}} 37^{\text{m}} 22^{\text{s}}.62$, we find—

$$2285 \text{ rotations} = 2344 \text{ days } 7^{\text{h}} 26^{\text{m}} 27^{\text{s}}.$$

a difference of nearly two hours. This would indicate, of course, a discrepancy of two hours between the indicated rotation-phases.

The just comparison is as follows :—

Huyghens	...	...	1672, August 13, $12^{\text{h}} 10^{\text{m}} 6$
Hooke	...	...	1666, March 13, $2^{\text{h}} 56^{\text{m}} 1$
Difference	...	...	2345 days 9 $14^{\text{h}} 5$

In the interval *Mars* made an exact number of rotations ; *but*, taking the period as $24^{\text{h}} 37^{\text{m}} 22^{\text{s}}.735$, we find

$$2286 \text{ rotations} = 2345 \text{ days } 8^{\text{h}} 8^{\text{m}} 12^{\text{s}}.$$

a difference of $1^{\text{h}} 6^{\text{m}} 18^{\text{s}}$.

This discrepancy is easily disposed of. Note first, that when the discrepancy is thus reduced, the supposition that Hooke depicted some other feature than the Kaiser Sea must at once be dismissed. Then, an error corresponding to 40 minutes' rotation may be readily assigned to Huyghens' drawing, and one corresponding to rather more than 26 minutes' rotation to Hooke's. This seems a fair apportionment of the discrepancy, and a very brief inspection of the figures will show that the difference in the drawings would be insignificant.

Now if we assign $26\frac{1}{2}$ minutes of the error to Hooke's drawing, setting the Kaiser Sea towards the right by the rotation in

that interval, we get for the rotation period $24^h 37^m 22^s.713$, instead of my former estimate, $24^h 37^m 22^s.735$. And this seems the result according best with the evidence; or one may say that the rotation period is fairly represented by the value, $24^h 37^m 22^s.71$.

It may be noted that if we ascribe the whole error to Hooke, we get the period $24^h 37^m 22^s.681$, whereas if we ascribe the whole error to Huyghens, we get the period $24^h 37^m 22^s.735$, and the period may be regarded as certainly lying between these extreme values (only differing by 0.054^s).

I conceive that had Kaiser detected the calendar errors in his computation, he would not have contended for the rejection of Hooke's observations. By applying to the ancient observations his method of taking the probable mean of modern observations, we deduce the rotation period $24^h 37^m 22^s.71$, or perhaps giving due weight to the fact that Hooke made two drawings, we may infer a value lying even nearer to my former estimate. When we note the roughness of Huyghens' drawing, and the unsuitability of the aspect he has pictured for deducing a time-estimate, we may not unfairly consider that a somewhat larger proportion of the error should be awarded to Huyghens. Hence we should deduce some such period as $24^h 37^m 22^s.72$. But the difference between this result and the other is not worth special discussion. The truth probably lies between these values,—

$$\begin{array}{r} 24^h 37^m 22^s.71 \\ \text{and} \quad 24 \quad 37 \quad 22 \quad .72 \end{array}$$

The Distribution of the Clusters and Nebulæ.

By Sidney Waters, Esq

The charts which I now beg to lay before the Society contain all the objects recorded in Sir John Herschel's general catalogue of 1864.

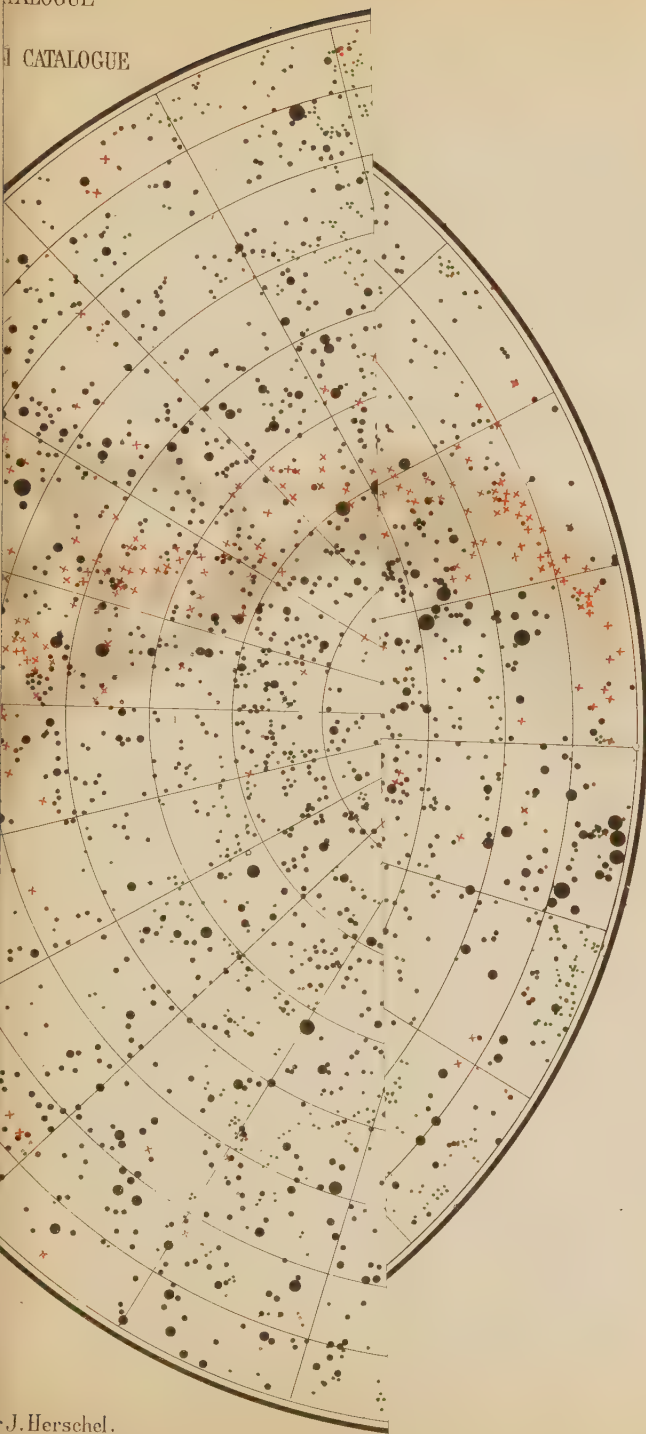
The projection is isographic, equal areas in the heavens being represented by equal areas *in plano*, and the objects are marked within one degree in P. D., and 4 minutes in R. A. of their true places.

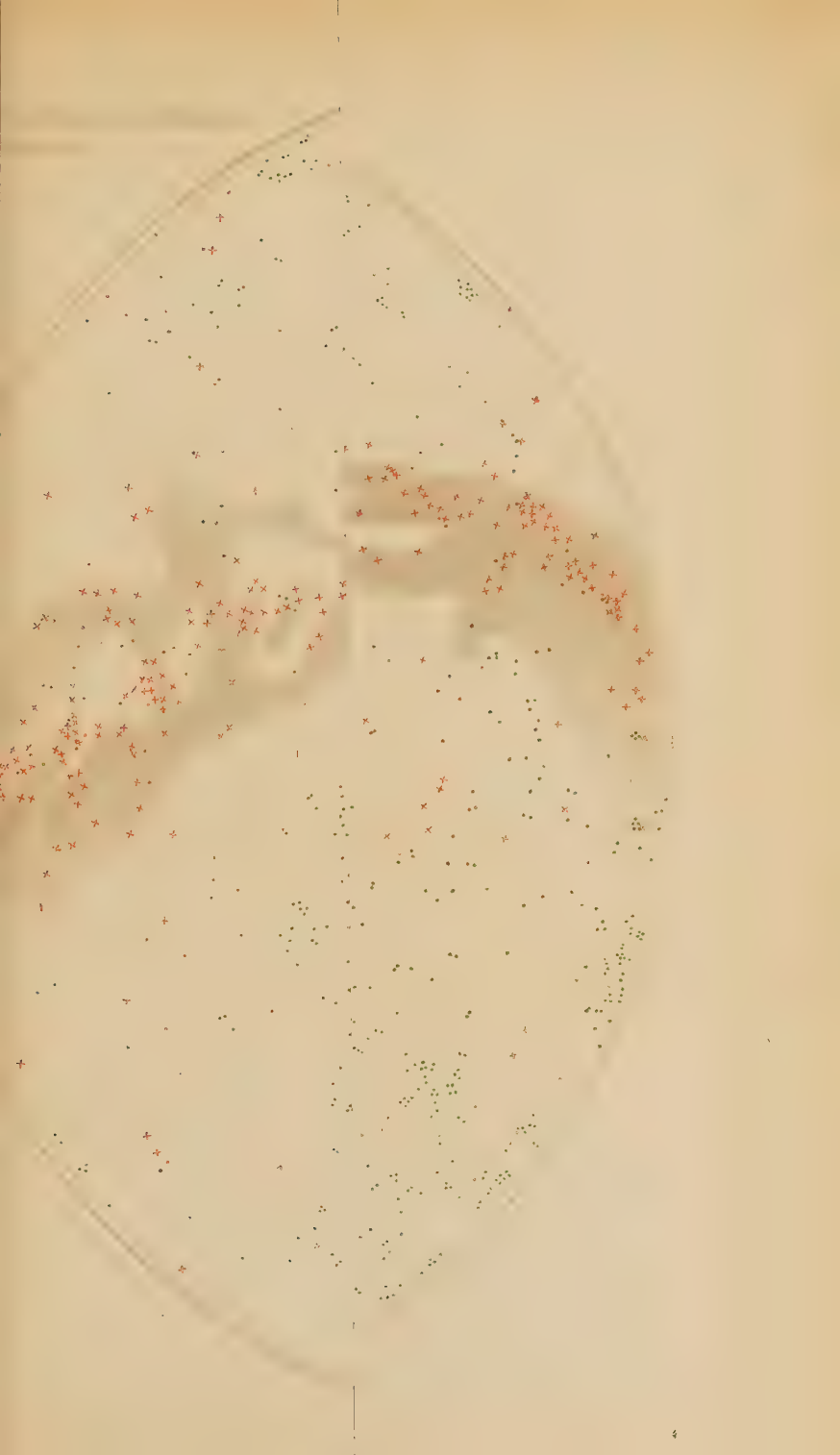
It will be seen that the contents of the Catalogue have been divided into three classes:—1st. Clusters (globular and irregular); 2nd. Resolvable Nebulæ; 3rd. Irresolvable Nebulæ; each class being distinguished in the maps by a distinctive mark.

A careful study of Mr. Proctor's Distribution Charts, published in the *Monthly Notices* in 1869, convinced me that by applying this method of charting, a further insight would be gained into the laws of nebular distribution, and I believe that

TALOGUE

I CATALOGUE





the charts now before the Society will be found to repay careful examination.

I do not now propose to enter into a critical discussion of their teaching, but shall content myself with pointing out two obvious conclusions which they seem to indicate.

1st. The coincidences in the apparent positions of the resolvable and irresolvable nebulae in the heavens are very significant; we not only find the great masses of resolvable nebulae coinciding in position with the far greater clusters of irresolvable nebulae in the neighbourhood of *Virgo* and *Coma*, but the streams and minor clusters of nebulae are invariably followed by streams and clusters of resolvable nebulae; and I believe that it is impossible to ascribe these peculiarities to chance.

The case made out by Whewell, and advocated by Mr. Proctor, that the *resolvability of a star group is no criterion of its distance* seems here to be established, as far as it can be established, by such means; it has long been certain that all orders of nebulae do exist commingled in the Nubeculae, it is now seen to be almost equally certain that they exist commingled in other parts of the heavens.

2nd. The great aggregation of clusters in the neighbourhood of the galaxy; this fact has often been noted before; but these charts seem to illustrate it most remarkably, and the conviction cannot be avoided that the clusters are part of, and most of them probably immersed in, the Milky Way itself; equally remarkable is the complete segregation of all the nebulae (the gaseous nebulae excepted) from the galactic zone.

These facts surely prove beyond question that not only are the clusters, which are peculiar to the Milky Way, related to the nebulae, which seem to form a distinct scheme, but that the two schemes are probably subordinate parts of our sidereal system.

Measures of the Diameter of Venus.

By John I. Plummer, Esq.

The accompanying series of measures of the diameter of *Venus* has been made with Airy's Double Image Micrometer upon every available occasion near the recent inferior conjunction of the planet. The greatest care has been taken to ensure a steady image, by equalising the interior and exterior temperature of the observing-room, and upon all those days when the definition of the planet was not considered sufficiently good, no observation has been attempted. On the other hand, none, after having been made, are rejected in the final results, all being assumed of equal weight. The value of a revolution of the micrometer-screw was determined from observations taken by myself upon four evenings in 1868, which leave no doubt as to the exactness of the assumed

equivalent. With the magnifying power that has been employed in these measurements (113) this equivalent is, 1 rev. = $18''\cdot693$. As the investigation of the correction due to irradiation was an important item in this inquiry, it was necessary that any change in this value depending upon temperature should be eliminated. This has been done simply and effectually by spreading the observations over that period of the year during which the temperature is continually increasing, the planet attaining its maximum diameter about the middle of the period. The results, however, do not appear to indicate that any sensible change actually exists. The measures have all been taken in full daylight, and when the planet was not far from the meridian, and are corrected for the difference of refraction of the two cusps.

Day of Observation.	Observed Diameter corrected for Refraction.		Calculated Diameter from Naut. Alm.		Obs.—Calc.	Log. Distance.	No. of Obs.	Int. Temp.
1878.	h	m	"	"	"			°
Feb. 18	2	50	23'115	22'524	+0'591	9'86772	10	45'5
	19	2	23'527	22'747	+0'780	9'86345	10	44'5
	22	2	24'312	23'470	+0'842	9'84986	11	42'7
	24	2	24'724	23'980	+0'744	9'84052	12	39'8
Mar. 2	3	0	26'593	25'669	+0'924	9'81096	10	44'8
	4	4	26'837	26'296	+0'541	9'80048	12	49'9
	5	5	27'081	26'631	+0'450	9'79498	10	45'2
	12	5	29'624	29'096	+0'528	9'75554	13	42'4
	26	2	36'315	35'392	+0'923	9'67146	12	51'5
	27	3	36'969	35'967	+1'002	9'66446	12	53'2
	28	1	37'381	36'471	+0'910	9'65842	12	52'2
	29	2	37'998	37'071	+0'927	9'65134	12	53'2
Apr. 18	1	45	52'094	50'498	+1'596	9'51710	18	55'3
	20	2	53'384	51'849	+1'535	9'50563	16	55'8
	20	23	53'499	52'458	+1'041	9'50056	16	56'4
May 1	0	0	59'152	57'368	+1'784	9'46170	12	60'0
	30	0	46'876	44'927	+1'949	9'56786	8	60'2
	30	23	46'062	44'240	+1'822	9'57455	12	58'5
June 8	23	45	39'406	38'201	+1'205	9'63829	10	65'3
	15	23	35'422	34'208	+1'214	9'68624	12	65'2
	16	23	34'336	33'699	+0'637	9'69275	12	68'0
	18	0	33'630	33'169	+0'461	9'69964	12	69'0
July 10	0	30	25'024	24'679	+0'345	9'82805	12	66'5
	20	23	21'991	21'853	+0'138	9'88086	12	76'8
	21	23	22'028	21'626	+0'402	9'88539	16	81'3
	22	23	21'393	21'406	-0'013	9'88983	12	81'0

If we divide the observations into two groups depending upon the distance of the planet from the Earth, it is clear that each observation will furnish an equation of condition, involving the

true diameter of the planet and the correction due to irradiation. Thus, if Δ = the distance of the planet at the time of observation; D, its true diameter at the distance of the Earth from the Sun; d , the angular diameter as measured; and x , the augmentation of the diameter from irradiation; then

$$D = \Delta (d - x).$$

Hence we have

July 23	21'393	- x	- 1'2888	D = 0
22	22'028	- x	- 1'3020	D = 0
21	21'991	- x	- 1'3156	D = 0
Feb. 18	23'115	- x	- 1'3561	D = 0
19	23'527	- x	- 1'3695	D = 0
22	24'312	- x	- 1'4130	D = 0
24	24'724	- x	- 1'4437	D = 0
July 10	25'024	- x	- 1'4858	D = 0
Mar. 2	26'593	- x	- 1'5454	D = 0
4	26'837	- x	- 1'5831	D = 0
5	27'081	- x	- 1'6033	D = 0
12	29'624	- x	- 1'7517	D = 0
June 18	33'630	- x	- 1'9969	D = 0
17	34'336	- x	- 2'0289	D = 0
16	35'422	- x	- 2'0595	D = 0
Mar. 26	36'315	- x	- 2'1308	D = 0
27	36'969	- x	- 2'1654	D = 0
28	37'381	- x	- 2'1957	D = 0
29	37'998	- x	- 2'2318	D = 0
June 9	39'406	- x	- 2'2999	D = 0
May 31	46'062	- x	- 2'6635	D = 0
30	46'876	- x	- 2'7048	D = 0
April 18	52'094	- x	- 3'0402	D = 0
20	53'384	- x	- 3'1215	D = 0
21	53'499	- x	- 3'1582	D = 0
May 1	59'152	- x	- 3'4538	D = 0

Since all the equations are of the same weight, D and x may be found by simply taking the means of the two groups, as follows:—

$$\begin{aligned} 25''3753 - x - 1'49653 D &= 0 \\ 43'7611 - x - 2'55800 D &= 0 \end{aligned}$$

From which we derive

$$\begin{aligned} D &= 17''321 & \text{Prob. Error} &= \pm 0''046 \\ x &= - 0'546 \end{aligned}$$

As the sign of the irradiation correction is *minus*, it follows that the contacts have been made too close by half the above quantity, or $0''.273$; this appears to be usually the case in day-light observations of *Venus*. It only remains to test the accuracy of the observations by substituting the deduced values of D and x in the preceding equations, and comparing the result with the original measures. In this manner we find for each day of observation:—

Date, 1873.	Computed Diameter.	Observed Diameter.	Obs. — Calc.
July 23	21'776	21'393	— 0'383
22	22'006	22'028	+ 0'022
21	22'242	21'991	— 0'251
Feb. 18	22'942	23'115	+ 0'173
19	23'174	23'527	+ 0'353
22	23'928	24'312	+ 0'384
24	24'460	24'724	+ 0'264
July 10	25'189	25'024	— 0'165
Mar. 2	26'222	26'593	+ 0'371
4	26'875	26'837	— 0'038
5	27'225	27'081	— 0'144
12	29'795	29'624	— 0'171
June 18	34'042	33'630	— 0'412
17	34'595	34'336	— 0'259
16	35'116	35'422	+ 0'306
Mar. 26	36'361	36'315	— 0'046
27	36'961	36'969	+ 0'008
28	37'486	37'381	— 0'105
29	38'111	37'998	— 0'113
June 9	39'290	39'406	+ 0'116
May 31	45'588	46'062	+ 0'474
30	46'304	46'876	+ 0'572
Apr. 18	52'113	52'094	— 0'019
20	53'522	53'384	— 0'138
21	54'157	53'499	— 0'658
May 1	59'277	59'152	— 0'125

It will be seen that the agreement is generally very good, and there is, I think, reason to believe that in the cases where the difference is sensibly above the average, that it arises from a variation in the amount of irradiation, depending upon the transparency of the atmosphere. Thus, upon the first five days of observation (February 18 to March 2) I have noted that the sky was unusually clear, and the measured diameter is rather too great. On the contrary, upon June 17, June 18, and July 23, the sky was hazy, and the measured diameter appears to be in a

slight degree too small. The value of x given above will, therefore, be understood to apply to the atmosphere in its mean state. Of the other cases of discordance, April 21, May 30, and May 31, I have no explanation to offer, except that the two last were scarcely so satisfactorily observed as the average of the measures.

Durham Observatory,
1873, August 13th.

Note on Logarithmic Tables. By Col. Tennant.

I have been glad to see that Mr. J. W. L. Glaisher has brought forward the necessity of some authority to superintend the publication of Mathematical Tables. I have long felt the want. Some such arrangement is necessary, not only to secure accuracy, but to allow the price to be reduced as far as possible.

Mr. Glaisher's researches have only extended as yet to logarithms of numbers, but those of circular functions are fully as important, and in offering a few suggestions, I propose to assume that both will be published and dealt with in the same manner.

It seems to me absolutely necessary that the second of space should be the difference of argument of the Tables of Circular Functions, and that the book of Tables should not be of enormous size. The labour of using Taylor's Tables is very great; Bagay's are not clearly printed, and the volume has a mass of matter in it which adds to its cumbrousness. I believe that for ordinary use seven figures are enough, and I know of no tables which approach Shortrede's, whether for convenience or accuracy. It is the custom in the Indian Survey to compute in duplicate with different sets of tables; in my experience Shortrede's were always preferred by the computers, and I believe that when corrected up according to the list of errata in vols. xxiv. and xxvii. of the *Monthly Notices*, they will be found singularly convenient and singularly accurate. I have never found the *nokta* inconvenient; it serves to mark the place where the third figure changes, and one knows instinctively that all the logarithms which follow in the same line have the first three figures following in the Table of Numbers, and at the bottom of the page in those of circular functions. In fact, it is only one *nokta* which is wanted. The figures are rather crowded, especially in the circular functions; but it is impossible to avoid this without increasing the size of the page, and incurring more inconvenience. I think if it were seriously proposed to publish standard 7-figure tables under authority, I should prefer to see Shortrede's taken as the pattern; and I offer the following suggestions as to improvements, in the hope that they might be found useful:—

First, if the stereotype plates now in existence be not used, and the tables be set up again, I would strongly recommend a more

uniformly thick figure ; next and last of general suggestions, I would propose a better paper, and that the size should be rendered unchangeable. It is very difficult to correct on unsized paper, except with a pencil, and equally so on a book which has been printed on such paper as has been used for Shortrede's Tables.

I think the following changes would be improvements :—

First, the circular arcs should be modified in the logs. of numbers. For this there are two plans. The indices in the first nineteen pages might be reduced by one, and the arcs throughout divided by ten. This would be generally more convenient than the present arrangement. A second plan would be to remove the indices in the first nineteen pages, which are not really necessary; the present arcs might then stand, and if the minutes corresponding to the tenth part were placed at the head of each column, it would not be difficult to take out the arc. I have had to enter minutes thus on my book, and find it easy to read; thus, $45^{\circ}86''$ stand in a column headed $76'$. The last figure of the seconds is 6, and as it is in the third group of ten, the arc is $76'-26''$. It would not be necessary to head with the number of minutes, however, if the following suggestions were adopted :—

The running heading of constants in each page seems to me of very little use. Suppose a column contains n' to $(n+1)'$, then I would give as the headings (in the two column pages) :—

$\sin n'$	$\sin (n+1)'$	$\sin (n+2)'$
$\sin n'$	$\sin (n+1)'$	$\sin (n+2)'$
$60\ n$	$60\ (n+1)$	$60\ (n+2)$
$\tan n'$	$\tan (n+1)'$	$\tan (n+2)'$
$\tan n'$	$\tan (n+1)'$	$\tan (n+2)'$
$60\ n$	$60\ (n+1)$	$60\ (n+2)$

It would be seen that the left-hand column was n' , and the right $(n+1)'$, and it would be easy to get the circular functions from the arcs, or the latter from the former up to $3^{\circ}20'$ without more than one opening of the page, except when arcs under $18'$ were wanted to several places of decimals.

I do not think that the logs. in the first or last columns of the heading are of much use ordinarily, and of those in the middle one I would remove $\log. 3^h$.

I have used the antilogarithms a good deal, but it is convenient to have the logarithms of numbers and those of circular functions in one volume, and I doubt if the inconvenience caused by the bulk of this table is not more than any gain from it. The other Tables from III. to XI. I would relegate to a book of special Tables. The only exception would be, I think, a part of Table VI. for obtaining hyperbolic logs from common ones, as

the former are often required in analysis. I do not think Shortrede's form of Gaussian logarithm convenient, and generally, I think, they would not be extensive enough.

I see little that could be done to the Table of Circular Functions. The extending it to the whole circle, and adding time arguments is a great convenience, and the only change I can think of as desirable is the extending the proportional parts for time, but the space seems to forbid.

The Table of Constants should be revised. With the change I have proposed in the running headings of the logs of numbers there would be no need for the columns $\frac{\sin}{\text{arc}}$ and $\frac{\tan}{\text{arc}}$. A great many of the constants might be dispensed with, and fifteen and thirteen places in the logs are quite needless. All the numbers should be carefully revised, and in selecting new ones I would avoid local numbers. Thus, gravity is given at Greenwich which is natural for an Englishman, though English physicists seem to refer now a great deal to Paris; but it would be preferable to give gravity at the Equator or Pole with the formula for correcting it, and the necessary logarithms. Many more constants and their logarithms could be added, even in one page, I think, if these changes were made. But the page should be much less crowded than at present, and the constants should be divided into groups, each with a conspicuous heading. Lastly, a few pages of good paper, for constants required by the owner in his pursuits, might be added with advantage.

I have long thought that 10-figure logarithms were wanted, both of numbers and circular functions; but I see no chance of such tables paying, for many a long day. They would only be used in particular cases.

I think in all computations where they are required it is impossible for a computer not to feel the enormous disadvantage of our mode of dividing the day and the circle. I have a strong feeling myself that the day and the circumference would be most conveniently decimally divided. I would make the difference of successive arguments in the Table of Circular Functions $\frac{1}{1000000}$ of the circumference. Ordinary men and their calculations would not be affected, and perhaps by the time they have advanced to 10-figure tables they will have got to appreciate decimal division. It would be easy to use the MS. French Tables to compare with any published on this system.

Note on observing Lunar Zenith-Distances for Longitude.
By Colonel Tennant.

The only rules I have ever seen for observing lunar zenith-distances for longitude are, first, that given by the Astronomer

Royal to the officers who were appointed to the North American Boundary Survey, which is about 6^h sidereal time, and for which he gives no reason; and secondly, that by M. Chauvenet, who says the Moon should be in the prime vertical. This is, I believe, wrong, nor does the Astronomer Royal's rule always answer.

If we suppose a perpendicular let fall from the zenith on to the great circle defining the lunar orbit, it will meet it in a point which (from analogy) I shall call the *nonagesimal of the lunar orbit*. The nonagesimal bisects that part of the orbit above the horizon, and is the highest point in it.

Since we know the time, and compute the Moon's altitude on the hypothesis of an assumed longitude, what we want to know is not the Moon's hour-angle at the moment of observation, but the amount by which her altitude is changed by error of longitude assumed, which changes her place in her orbit. When the orbit coincides with the vertical circle, this change is greatest, *ceteris paribus*, and in all other cases the less the inclination of the vertical to the orbit the greater the effect of change of place on the altitude.

If the station of observation have a latitude less than the greatest declination of the Moon in that lunation, then there are two sidereal times when the Moon's orbit passes through the zenith, and these will be found without difficulty by finding the right ascensions of the Moon when her declination equals the latitude.

At any other time the zenith distance of the nonagesimal will be the least inclination of the orbit to the vertical, and it measures this amount at the point of the orbit which is in the horizon, and whose distance both from nonagesimal and zenith is 90°.

If the latitude be beyond the limits of the Moon's declination, then the least of all the possible inclinations is when the orbit is perpendicular to the meridian, which is when the sidereal time = 6^h + A.R. of γ 's orbit on equator.

When the orbit from any cause cannot pass through the zenith, the Moon should be low during observation. When it does, the zenith distance is immaterial. If the sidereal time be earlier than the best time by the last paragraph, then the zenith distance of the nonagesimal of the Moon's orbit is decreasing, and the best time would be evidently *after* she has risen. If there be a choice, she should be therefore observed in the east. If the sidereal time be later, she should be observed in the west, but the zenith distance should not in any case be small.

It is possible, of course, to give a formula by which the theoretically best sidereal time shall be got for any given position of the Moon and her orbit, but I do not think this would be of any practical use.

There is a practical point in Chauvenet's book which I wish could be remedied. In it, as in all investigations I have seen, the equation of condition for eclipses and occultations, &c. contains a

term involving the error of eccentricity of the terrestrial meridian. I need hardly say that the amount of doubt on this point is so small now that such observation could not be affected. But there has of late years arisen a very serious doubt as to the accuracy of geometric latitudes deduced from the astronomical or observed values. I think it is generally believed that there is everywhere some deflection of the plumb-line from the place it would have were the Earth constituted by law, and that in many places this amount is of importance.

We have no means of determining an absolute amount of meridian deflection, or a real geocentric latitude, except by the changes which the use of a wrong one produces in the resulting places of the Moon. I have before called attention to this (*Monthly Notices*, xvii. 62, paras. 14-15), and I would now propose that in a new edition of Chauvenet, or in any new work, the useless equation should be omitted, and that in lieu should be given two; one for the correction to the geometric latitude, and the other for a correction to the calculated radius vector of the Earth, which I have also shown (*Monthly Notices*, xvii. 236), may in some cases be sensibly affected.

Errors and Omissions in the Catalogue of Sir William Herschel's Double-Stars. By S. W. Burnham.

In the preparation of a general catalogue of double-stars I have had occasion to examine carefully Sir John Herschel's catalogue of his father's double-stars (*Memoirs of the Royal Astronomical Society*, vol. xxxv.), in connexion with various other catalogues, and in so doing have detected some errors, a record of which may be of service to double-star observers.

Sir John Herschel has undertaken to give in the column of "Synonyms and Remarks," the corresponding number of the double-star where it is found in the catalogues of Struve (*Mensuræ Micrometricæ*); Herschel and South (*Phil. Trans.* 1824); and South (*Phil. Trans.* 1826); but in many instances this has been overlooked. A number of them are also included in Sir John Herschel's own catalogues published in the various *Memoirs* of the Royal Astronomical Society.

Class I.

No. 13 (*Aquilæ* 136), for Σ 2541, read Σ 2525; and for S. 770, read S. 720.

36 (ξ *Herculis*), add Sh. 237.

48 This is identical with a double discovered by Alvan Clark in 1859, and heretofore regarded, by Dawes and others, as new. Herschel in 1783 made the angle $259^{\circ}8$; Dawes in 1859 found it $246^{\circ}1$; and it decreased by his measures about 2° in the seven years following (*Memoirs of the Royal Astronomical Society*, vol. xxxv. p. 468.) It is No. 103 of Chambers' *Catalogue of Binary Stars*.

- 63 Add S. 778.
 64 (π *Arietis*), for Sh. 82, read Sh. 35.
 81 (*Serpentis* 112), there can be no doubt of the identity of this triple with Σ 1990 (= S. 675.) Struve gives for the wide pair $D = 56''.17$; $P = 59^\circ$; and Herschel's measures are $D = 56''.47$; $P = 238^\circ.2$ ($58^\circ.2$.) Struve's position angle of the other star is 209° ; and Herschel's, 210° .
 84 Add S. 526.
 91 Add S. 723.
 94 (δ *Cygni*), add Sh. 304.

Class II.

- No. 21 Add Sh. 215.
 32 This is identical with H. N. 84, the places of the two, and the distance agreeing exactly.
 42 This double is in one of Sir John Herschel's own catalogues of double-stars (= H 219.)
 62 This is H. 3041 = O Σ 443.
 66 Add S. 764.
 70 Add S. 734.
 72 Add S. 531.
 86 Add Σ 2016.
 97 Add S. 775.

Class III.

- No. 16 Add S. 751.
 17 Add Sh. 341.
 28 "Near γ *Argûs*; place very doubtful." This star is Lalande 15389; it follows γ *Argûs* $41''$, and is $1' 26''$ north.
 48 One of Sir John Herschel's doubles (= H. 422.)
 71 Add S. 795.
 110 "Identification uncertain." This triple is O Σ 447, the measures corresponding very closely.
 113 *A* and *B* of this quadruple constitute Sh. 314; and *D* and *E* Sh. 315.

Class IV.

- No. 15 (*Camelop.* 212), for Σ 1674, read Σ 1694.
 22 (f^2 *Cygni*), add Σ 2743.
 23 (ω^3 *Cygni*), this is not ω^3 *Cygni*, but is P. xx. 199; add S. 755, and for Σ C. P. 684, read Σ C. P. 683.
 24 This is ω^3 *Cygni* (= H. 1534), for Σ C. P. 682, read Σ C. P. 684.
 45 Add Sh. 57.
 71 (α *Capricorni*), add Sh. 324.
 73 There is no doubt of the identity of this with Σ 587.
 77 This is not Sh. 73 as stated.
 92 Add S. 759.
 94 Add Sh. 273.

- 127 This is not Sh. 285, that pair being Σ 2434, nor is it P. xviii. 274, unless Struve has erred in attaching that number from Piazzì to his pair. The two double-stars are in the same vicinity, Herschel's being the brightest and perhaps correctly designated from Piazzì.
- 132 Erroneously stated to be the same as H. N. 62. The two differ in Right Ascension more than three hours.

Class V.

- No. 1 (δ *Herculis*), add Σ 3137.
- 7 (μ *Sagittarii*), this is also H. 2822 (*Cape Observations*.)
- 18 (θ *Cassiopeiæ*) = H. 1993.
- 51 (θ *Hydræ*) = H. 2489.
- 59 (θ *Cancrì*) = H. 2452.
- 72 (36 and 37 *Herculis*), add Sh. 24.
- 76 (β *Aquarii*) = H. 936.
- 80 (τ *Aquarii*), add Σ 2943 and S. 817.
- 105 Identical with O Σ 397.
- 115 (σ *Tauri*), this is H. 365 with two other companions.
- 122 (*Boötis* 346), for Sh. 119, read Sh. 189 (= O Σ 291.)
- 131 (*Libræ* 97), for Σ 476, read Σ C. P. 476, referring to Struve's early catalogue (*Dorpat Obs.* 1822.)

Class VI.

- No. 29 (ϵ *Capricorni*), for H. II 57 (the close pair), read H. II 51.
- 55 (2 *Cassiopeiæ*), add S. 823.
- 57 (79 *Cygni*), this is not included at all in the synoptical catalogue which follows where the several classes are arranged in order of Right Ascension.
- 96 (ξ *Persei*) = H. 337.

145 New Double-Stars.

- 11 Add Sh. 352.
- 13 Add Sh. 303.
- 33 (*Libræ* 178), add Sh. 206.
- 36 (35 *Sextantis*), add Σ 1466.
- 42 (13 *Lacertæ*) = H. 1803 = O Σ 479.
- 61 (20 *Lyncis*), add Σ 1065.
- 98 This is omitted in the catalogue arranged in order of Right Ascension (= Σ 1813)
- 109 Add Sh. 30.
- 121 The N.P.D. of this pair ($19^{\circ} 4'$) is obviously wrong, as from the description it must be in *Aquarius*.
- 125 The measures are given, it being simply noted as Class I. In looking for new doubles I recently found this, and estimated $P = 275^{\circ}$
 $D = 2''.5$. It is Lalande 34048, 1^m following and $21'$ north of λ *Sagittarii*.

115 (*Bootis* 76), add S. 660.

123 This is 19 *Canis Majoris*. The distance is about 10". Given as Class II.

138 Recorded without measures as Class I. Peters in observing a minor planet found and measured a double-star which is undoubtedly identical with H. N. 138. He gives $P = 331^{\circ} 8$; $D = 4'$ (*Astron. Nach.* 1635.) The place agrees closely with Herschel, and I have not been able to find any other double-star in the neighbourhood.

In the catalogue in order of Right Ascension, general number 289 (H I. 24), the North Polar Distance given as $73^{\circ} 59' 24''$ should be $71^{\circ} 59' 24''$; and No. 541 (H V. 11), given as $38^{\circ} 44' 3''$ should be $34^{\circ} 44' 3''$.

On the most Probable Result which can be derived from a number of direct Determinations of Assumed Equal Value. By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer at the Cape.

Let $x_1 x_2 \dots x_n$ be n direct measures of the same quantity: each apparently equally good and, by assumption, to be considered as each equally probable. Each measure is therefore, *a priori*, by assumption equally likely to be the true result; each is equally likely by the assumption to differ from the true result by an assigned quantity. Positive and negative errors therefore must be considered as equally probable to the same amount: for the greatest or least of the direct measures is, by assumption, each equally likely to be the true result.

I assume as an axiom that since all the direct measures are by assumption of equal value, or equally good, the most probable value which can be adopted is that to which each individual measure equally contributes. To obtain the most probable value, therefore, we must combine all the independent measures in such a way, that an error which may exist in one of the measures, as x_1 , shall produce the same error in the "value adopted as the most probable" as would be produced by the same error in $x_2 x_3$ or x_n .

This appears to me clear. The probable discordance of each measure from the true result is the same, and this being the case, no good reason can be assigned why we should adopt a value in which an existing error, or arbitrary change, in x_1 should produce either a greater or less error, or arbitrary change, in the adopted value than would be produced by the same error or arbitrary change, in $x_2 x_3$ or x_n . This condition of equal contribution of the independent measures to the most probable result appears to me necessary and sufficient.

Let $u = \phi(x_1 x_2 \dots x_n)$ be the value adopted as the most probable.

Then since equal errors, or changes, in u are to be produced by the same error, or change, in $x_1 x_2 \dots$ or x_n , we must have the following conditions satisfied amongst the partial differential coefficients,—

$$\frac{d u}{d x_1} = \frac{d u}{d x_2} \dots = \frac{d u}{d x_n}$$

$$\frac{d^2 u}{d x_1^2} = \frac{d^2 u}{d x_2^2} \dots = \frac{d^2 u}{d x_n^2}$$

Therefore

$$\frac{d^2 u}{d x_1 d x_2} = \frac{d^2 u}{d x_1^2} = \frac{d^2 u}{d x_2^2} = \&c.$$

Hence, generally

$$\frac{d^{r+s} u}{d x_1^r \cdot d x_2^s} = \frac{d^{r+s} u}{d x_1^{r+s}} = \frac{d^{r+s} u}{d x_2^{r+s}} = \&c.$$

Now let $x_1 = a_1 + h_1$, $x_2 = a_2 + h_2$, $x_n = a_n + h_n$. Then by comparison

$$u = \phi(a_1 a_2 \dots a_n) + \left(h_1 \frac{d}{d x_1} + h_2 \frac{d}{d x_2} + \dots \right) \phi + \frac{\left(h_1 \frac{d}{d x_1} + \dots \right)^2 \phi}{1 \cdot 2} \\ + \frac{\left(h_1 \frac{d}{d x_1} + h_2 \frac{d}{d x_2} + \dots \right)^r \phi(a_1 + \theta h_1, a_2 + \theta h_2, \dots a_n + \theta h_n)}{r}$$

$\therefore$ by conditions A

$$u = \phi(a_1 a_2 \dots a_n) + (h_1 + h_2 \dots + h_n) \frac{d u}{d x_1} \\ + \frac{(h_1 + h_2 \dots + h_n)^2}{1 \cdot 2} \frac{d^2 u}{d x_1^2} + \frac{(h_1 + h_2 \dots + h_n)^r}{r} \frac{d^r \phi}{d x_1^r} (a_1 + \theta h_1, \dots a_n + \theta h_n)$$

Let

$$a_1 = a_2 \dots = a_n = s = \frac{1}{n} (x_1 + x_2 \dots + x_n)$$

Then

$$x_1 = s + h_1, \quad x_2 = s + h_2, \quad x_n = s + h_n$$

But

$$h_1 + h_2 \dots \dots \dots + h_n = 0$$

therefore

$$u = \phi(s s \dots s) = F(s.)$$

or u is a function of the arithmetical mean. We cannot therefore adopt a value u equally dependent upon the independent measures

$x_1 x_2 \dots x_n$ except it can be expressed as a function of the arithmetical mean.

But when there are only two independent measures as x_1 and x_2 we know that the most probable result is $\frac{x_1 + x_2}{2}$ since no reason can be assigned why we should adopt a value nearer to x_1 than to x_2 .

$$\therefore F\left(\frac{x_1 + x_2}{2}\right) = \frac{x_1 + x_2}{2} \text{ or } F(s) = s \text{ when } n = 2.$$

Then assuming generally for n observations or measures

$$F\left(\frac{x_1 + x_2 \dots + x_n}{n}\right) = \frac{x_1 + x_2 \dots + x_n}{n}$$

or $F(s) = s$ for n measures.

We have

$$\begin{aligned} F\left(\frac{x_1 + x_2 \dots + x_{n+1}}{n+1}\right) &= F\left(\frac{ns + x_{n+1}}{n+1}\right) \\ &= F\left(s + \frac{x_{n+1} - s}{n+1}\right) \\ &= F(s) + \frac{x_{n+1} - s}{n+1} F'(s) + \left(\frac{x_{n+1} - s}{n+1}\right)^2 \frac{1}{1.2} F''(s) + \dots \end{aligned}$$

But $F(s) = s$ by assumption, $\therefore F'(s) = 1$, $F''(s) = 0$

$$\therefore F\left(\frac{x_1 + x_2 \dots + x_n + x_{n+1}}{n+1}\right) = s + \frac{x_{n+1} - s}{n+1} = \frac{ns + x_{n+1}}{n+1}$$

$$F\left(\frac{x_1 + x_2 \dots + x_n + x_{n+1}}{n+1}\right) = \frac{x_1 + x_2 \dots + x_n + x_{n+1}}{n+1}$$

$\therefore$ if the law hold for n measures it is proved for $(n+1)$; but it has been shown to be true for $n = 2$.

$\therefore$ by successive inductions it can be shown to be generally true.

That is, the most probable result which can be deduced from the n independent direct measures $x_1 x_2 \dots x_n$ is the arithmetical mean provided *we assume that each of these measures is equally probable*. If we give up or change that assumption then the proposition is no longer necessarily true.

The law of frequency can of course be at once deduced from the above result.

A Mechanical Representation of a Familiar Problem.

By Simon Newcomb.

One of the commonest of astronomical problems is this; Given at several epochs, observed values of a quantity which varies uniformly with the time, to find by least squares the most probable values of the two constants which fix its value at any time. The correction of the right ascension or declination of a star from all the observations made on it affords a familiar example of this problem. The mechanical representation alluded to is the following.

Plot the observed values of the quantity sought on a plane where the abscissæ shall represent the times, and the ordinates, taken on a very small scale the values observed. Let a straight rigid rod be attracted by each of these points with a force proportional to the weight of the corresponding observation multiplied by the distance of the point from the rod. Then:—

(1.) The position of equilibrium of the rod will be that corresponding to the most probable course of the variable quantity, the perpendicular distances of the point from the rod representing outstanding errors of observation.

(2.) The weight of the result at any time is measured by the resistance which the rod offers to pressure at the point corresponding to the time, or, by the weight of the observation, the attraction of which would be equal to the force with which the rod resists pressure.

(3.) The point of greatest weight, or least probable error, is that where the application of a force will cause the rod to move parallel to itself, or that round which the rod will turn under the influence of a couple.

(4.) To find the relative influence of the various observations in determining the computed value of the variable quantity at any epoch, apply a pressure to the rod at the point corresponding to this epoch. Observations at the point around which the rod turns in consequence of this pressure will be without influence, and the latter will be proportional to the distance of the epoch of observation from this point, whether before or after.

(5.) The points of pressure and of rotation are conjugate, *i.e.* if a pressure at P causes the rod to turn round Q, a pressure at Q will cause it to turn round P. This corresponds to the logical proposition that if an hypothesis that a star has any assumed right ascension at the epoch P is without influence upon our conclusion as to its right ascension at the epoch Q, then, no change in the latter result can affect the probability of the former.

Note on a Mechanical Representation of some cases in the Method of Least Squares.

(Supplement to Prof. Newcomb's Note on a Mechanical Representation of a Familiar Problem.)

The solution by least squares of a system of linear equations containing not more than three unknown quantities, may be represented in a way somewhat similar. If we conceive the three unknown quantities to represent rectangular co-ordinates in space, we may conceive each equation as demanding that the point sought shall be in the plane which it represents. When there are more than three equations, these demands cannot, in general, all be satisfied. But, let us suppose each plane, of which the equation is,

$$a x + b y + c z + n = 0; \text{ Weight} = w;$$

to attract the point with a force proportional to the quantity,

$$w (a^2 + b^2 + c^2),$$

and directly as the distance from the plane. The co-ordinates of the position of equilibrium of the point will then represent the values of the unknown quantities given by the solution by least squares.

If we suppose all the equations made homogeneous in weight by being multiplied by $\sqrt{w}$, the attractions of the plane,

$$a x + b y + c z + n = 0,$$

upon the point of which the co-ordinates are X, Y, Z , resolved in the direction of the three co-ordinate axes are :—

$$X = a (a X + b Y + c Z + n),$$

$$Y = b (a X + b Y + c Z + n),$$

$$Z = c (a X + b Y + c Z + n).$$

It will be seen that these equations are those by the summation of which the "normal equations" are formed in the method of least squares. Consequently the functions of X, Y , and Z , which are equalled to zero in these equations, represent the resolved attractions upon the point when we suppose X, Y , and Z , to represent its arbitrary co-ordinates.

The ideas suggested in this note may be considered as in some sort supplementary to those in a very elegant disquisition by Professor Donkin in *Lionville's Journal*, Tome xv., in which the general problem of least squares is considered as one of equilibrium.

*Reduction of Observations of Geographical Positions by
Dr. Livingstone.* By Sir T. Maclear.

(From a Letter to the Astronomer Royal for Scotland.)

At present I am engaged in the reduction of a batch of observations for geographical positions forwarded to me by Dr. Livingstone per Mr. Stanley.

While Livingstone was delayed at one place, partly from illness, he observed 29 sets of lunar distances for the longitude of that place.

The distances are reduced direct by trigonometry, and where trustworthy observations for time are available, I calculate the altitudes for each mean of distance measure instead of employing his observed altitudes fore and aft. This step more than trebles the labour, but it repays in accuracy.

I am just about grappling with an important locality, where he was prostrated by illness for a length of time, and lost some three weeks of reckoning. There is no difficulty, however, in finding the day and hour (with the aid of the *Nautical Almanac*), where he observed a lunar.

He started from Unyanyembe last August (1872) to skirt the westward of the lakes he discovered, to ascertain if any of them give out rivers to the west in the direction of the Congo, as suspected by geographers at home: likewise to verify or disprove the existence of a mountain from which four great rivers emerge, one of them being the Nile. On this occasion he will be in greater danger of personal violence than hitherto, provided the news of Sir Bartle Frere's mission to the East Coast, to negotiate for the suppression of slavery, becomes generally known in the interior.

*Spectroscopic Observations of Meteors at the Oljyalla Observatory
Hungary.* By Herr Nicolas de Konkoly.

(Communicated by John Browning, Esq.)

Last July I received a very fine meteor spectroscope from Mr. John Browning. I observed with it several meteors on the nights of the 25th and 26th of July. Before I observed with the instrument, I experimented with it on the spectra of the light of sky-rockets let off at a short distance from me, for determining the position of the lines of several gases. I regret I was not able to repeat the observations on the following night, but my assistant, Mr. Nagy, made some observations. I found three meteors give the following spectra. First: in the nucleus, the spectrum was continuous; in the train I did not see anything but the bands of sodium. The second meteor I observed gave the same spectrum as the first. The third meteor I observed, the spectrum of

the nucleus was the same as the spectrum of the first and second ; but green predominated in the spectrum. The meteor itself was emerald-green. In the train I saw the sodium band, and the band in the magnesium region about 1650 of Kirchhoff. The positions of the three meteors were as follows:—

	Beginning.	End.	
$12^h 34^m 23^s$	$\left\{ \begin{array}{l} \text{A.R. } 6^h 27^m 13^s \\ \text{D. } + 55^\circ 57' \end{array} \right.$	$\left\{ \begin{array}{l} 8^h 27^m 59^s \\ 57^\circ 1' \end{array} \right.$	1st meteor.
$12^h 55^m 0^s$	$\left\{ \begin{array}{l} \text{A.R. } 21^h 31^m 29^s \\ \text{D. } + 7^\circ 2' \end{array} \right.$	$\left\{ \begin{array}{l} 22^h 12^m 30^s \\ 25^\circ 39' \end{array} \right.$	2nd „
$13^h 30^m 30^s$	$\left\{ \begin{array}{l} \text{A.R. } 0^h 44^m 54^s \\ \text{D. } + 3^\circ 34' \end{array} \right.$	$\left\{ \begin{array}{l} 22^h 37^m 51^s \\ 27^\circ 12' \end{array} \right.$	3rd „

The time given is mean time at the Observatory: it is $7^m 24^s$ east of Vienna.

On the 25th July Mr. Nagy observed 65 meteors; on the 26th, also 65; on the 27th, 93; on the 28th, 18; on the 29th, 49 meteors, with the meteor spectroscope: in five days 286 meteors were observed; of these, 9 were as large as *Venus*; 35 first magnitude; 53 second magnitude; 58 third magnitude; 68 fourth magnitude; 44 fifth magnitude; and 19 sixth magnitude.

August 12th, 1873.

Views of the Ancient Rabbins relative to the Dimensions of the Earth. By A. D. Wackerbarth.

On the 14th of November, 1862, the Society did me the honour of reading a paper of mine relative to the history of the theories entertained in different ages concerning the spherical form of the Earth and its rotation on its axis. In that essay I endeavoured to show, that the Rabbins of the Cabbalistic School, *e.g.* the author of the *Zohar* (Simeon ben Yochai)*, and the author of the *Imre Binah*, entertained just notions both as to the figure and rotation of our planet. I would now add that these old Jewish teachers were also in possession of a very fair approximation to the dimensions of our globe.

The Greek or Egyptian land-surveyors' cubit has been determined by Sir Henry James with (as I believe) all possible accu-

* This truly great writer was an ascetic and recluse, who is said to have lived with his son for twelve years in a cavern, where God caused a fountain to spring up and a *Ceratonia Siliqua* tree (? Bread-fruit) to grow for their sustenance. For the sake of economy in clothes they did not dress themselves till the hour of evening prayer, but, to avoid the scandal of nakedness, studied all day buried up to their necks in sand. Epoch, about that of the second temple's destruction.

racy to be 18.2405 inches (English). The Hebrew ordinary cubit will therefore be 6 palms, *i. e.* six-fifths of this, or 21.8886 inches = 1.82405 feet, which agrees with Arbuthnot's estimate, 1.824.

In the treatise, *Pesachim*, פְּסָחִים of the Talmud, fol. 94 recto, we find it stated, that the circumference of the Earth is 6000 פָּרָסִין, or Parasangs.* Now a Parasang, פָּרָסָה, is = 12,000 זַמּוֹת (or זַמִּין, for both forms of the plural occur), or cubits; whence we have,

$$\text{One זַמּוֹת, or Cubit} = 2 \cdot \pi \cdot \frac{\text{Radius of Earth}}{72,000,000}.$$

The latest and, I believe, most reliable determination of the dimensions of the Earth, that I know of, is that of the English *Topographical Corps*, published by Colonel Clarke. If we take the equatorial semidiameter there given as our radius of the Earth, we have

$$\begin{aligned} \text{Cubit} &= 2 \cdot \pi \cdot \frac{20,926,062}{72,000,000} \text{ ft.} \\ &= 1.82572, \end{aligned}$$

differing from the true value, 1.82405, determined by Sir Henry James only by 0.00167 = 0.00167, or the 50th part of an inch.

To make an exact agreement would require a radius of 20,906,885 ft., which is somewhat greater than the mean between Col. Clarke's equatorial and polar semi-axis.

This, however, at any rate seems to show that the ancient Rabbins were in possession of a very fair approximation to the dimensions of the Earth; one, in fact, that comes within about 19,000 feet of the true length of the radius; but from what source they had obtained their information is more than I am able to explain.

Upsala, 1873, July 25.

Observations of a portion of the Moon's limb, not on the Sun's disk, during the late Solar Eclipse. By Henry Pratt.

The attention of the last meeting having been drawn by Capt. Noble to a hitherto unrecorded phenomenon observed by him during the partial eclipse of May 26th, 1873, a few additional notes of a similar and independent observation made by myself may not be without interest.

During the Eclipse the Sun was occasionally obscured by clouds for short intervals, often its light was diminished by a thin veil of haze, and again at other intervals it was perfectly clear

* שְׁתַּא אֶלְפֵי פָּרָסִי הוּא עֵלְמָא.

and well defined. The telescope chiefly used was a silvered-glass reflector, one of With's most exquisite specula of $8\frac{1}{8}$ -in. aperture in conjunction with one of Browning's perfect planes *unsilvered*; and which is at once a most simple and effective arrangement for solar observation, being the equivalent of a Hodgson eye-tube with a less number of reflecting surfaces. This means together with the tarnished state of my speculum reduced the light so that *no dark glass was needed*. The outline of the Moon's edge projected on the solar disk was of course finely seen, some of the peaks of the Dörfel Range being recognised by their profiles. After the eclipse had passed its greatest phase, the Moon's limb was seen to be traceable for some distance beyond the Sun's disk on the eastern side, and in a less degree on the western side. It was first detected with a Kelner power 85, *without a dark glass*, and its visibility estimated to extend beyond the Sun for a distance of about five minutes of arc on the eastern side. Beyond this distance it faded from view, was at no part very easily to be seen, but most so nearest to the Sun. A blue glass was now applied through which it was still visible, but not to so great an extent as without it, probably merely on account of the diminution of light. The eye-piece was now exchanged for a Kelner 45, *with the dark glass*, the phenomenon being still traceable, but not so clearly so as with 85. The effect of different eye-pieces was tested by substituting a Ramsden 170 *without* and *with a red glass*, and afterwards a Huyghenian 180, *without* and *with a wedge of neutral tint*, always with the same result, with the exception of variations in the extent of the Moon's limb rendered visible. The power 85 was the most efficient.

A small achromatic telescope by Cooke of 2.5 in. aperture was now turned on the object. It was armed with a Hodgson plane and a Huyghenian eye-piece 70 *with a neutral tint dark glass*, and afterwards with another negative eye-piece 160 *without a dark glass*. The phenomenon was as certainly perceived, but neither so easily nor for so great a distance from the Sun as with the larger telescope. It was observed alternately in the two instruments until the termination of the eclipse, when a cloud intervened for a few seconds, and after it had cleared off not the slightest trace of the Moon could be perceived. Whatever the cause of the appearance may be it is thus proved to be not of instrumental origin. And it is as remarkable that it became invisible at a short distance from the Sun as it is that it was most easily perceived close to that luminary's limb.* Very probably a large aperture and a weak light without a dark glass

* This seems to accord with the only explanation that can (I conceive) be suggested; viz., that the Moon was seen projected on the light of the solar corona, rendered thus discernible *by its effects*. Mr. Pratt's observations appear to me of great value and interest; and I would venture to invite special attention to the importance of applying, whenever practicable, such tests as he employed to show that the phenomena described were not of instrumental origin.—R. A. P.

may have been conditions greatly assisting me in the observation. It may be noted that a young friend who was with me at the time also saw it in the large telescope, but could not see it in the small one.

18 Preston Street, Brighton.

Note on the Preparation of Speculum Metal, by R. L. J. Ellery, F.R.S., the Observatory, Melbourne.

(Communicated by J. Browning, Esq.)

In a letter I have received from Mr. Ellery, the following remarks referring to reflecting telescopes, and the preparation of speculum metal, are so interesting that I think they will be considered worthy of a place in the Proceedings of the Society.

Mr. Ellery says, "That for amateurs and many others glass specula up to 10-in. or 12-in. diameter will nearly always be preferred : but that for dimensions greater than these metal would be found the best for many reasons. First, among these is the difficulty of supporting large glass surfaces, and the greater effect strains, whether from defective annealing or defective support, would have upon glass.

"I believe metal surfaces would last almost any time if the metal itself were properly made, and with only ordinary care taken of the polished surface.

"My attention has been drawn to a very important point as regards speculum metal by a gentleman here who has produced a composition the whitest, densest, and most difficult to tarnish I have ever seen, and he has effected this by the most scrupulous avoidance of contact with any iron while the metal was in a molten state. I could not have believed that merely stirring with an iron rod would make such a difference as it does. The metal that has come in contact with iron is always yellower or browner than that which has not, and certainly tarnishes much quicker."

A Method for determining the Heat-radiation from the Solar Spots. By Dr. Lohre, of the Bothkamp Observatory.

(From a Letter to the Foreign Secretary. Translation.)

The importance which a correct answer to the question, "Do Sun-spots exhibit a marked difference of heat-radiation from that of the surrounding solar surface?" has upon our interpretation of the processes going on in the Sun induced me to search for some easier way of solving this problem than the ordinary thermo-electric method. It occurred to me that some chemical compounds, which, as is well known, exhibit under a very moderate heat a

visible change of their chemical constitution or molecular structure, might be applicable for this purpose. Chloride of cobalt, as the most easily obtained, was first selected for trial. Subsequently I found that this salt is to be preferred for several reasons. The experiments made at present do not authorise me to consider the above question as definitively answered; but I will describe briefly the method of making them, and the result I obtained, in the hope that the very simple process may be tried when a large spot appears on the sun.

A sheet of the finest grained white paper is to be floated for four minutes on a solution of one part of crystallised chloride of cobalt to three parts of water, and then dried by exposure to the air. This prepared paper, which is of a pale red colour, is stretched on a frame which can be fixed at the focus of the 11-in. refractor of this observatory. This paper is then exposed for about two minutes to the Sun's image, the full aperture of the telescope being used.

A well-defined blue image of the Sun appears on the paper, in which the diminution of heat-radiation at the Sun's limb is plainly seen. Up to the present time no trace could be discovered of the *small* spots on the Sun. It is, therefore, important that the experiment should be repeated when a large spot presents itself. Unfortunately since the end of May the weather has been unfavourable and no large spot could be observed.

Discovery of Minor Planet (133) by Prof. Watson.

On the evening of August 18, the Astronomer Royal received a telegram from Professor Henry, of the Smithsonian Institution, announcing the discovery by Mr. Watson, of Ann Arbor, of a new Minor Planet (133). It was observed on the night of August 19 at Marseilles and Vienna.

The following observations were made by M. Stéphan at Marseilles:—

	Marseilles Mean Time.	R.A. (133).	$\pm$ (par. $\times \Delta$),	N.P.D. (133).	$\pm$ (par. $\times \Delta$).	Star of Comp.
1873.	h m s	h m s		° ' "		
Aug. 19	14 10 39	23 0 38.20	+9.079	92 43 30.6	-0.8059	<i>a</i>
20	12 36 48	22 59 56.30	-8.682	92 45 30.7	-0.8069	<i>b</i>
21	13 29 15	22 59 6.67	+8.775	92 47 41.7	-0.8073	<i>c</i>

Mean Position of the Comparison Stars for 1873.0.

		Mag.	R.A.	N.P.D.
			h m s	° ' "
<i>a</i>	W. B. xxii. 1237	9	22 59 26.99	92 46 22.7
<i>b</i>	*	9	22 58 54.69	92 45 32.8
<i>c</i>	W. B. xxiii. 3	8.9	23 2 31.10	92 56 36.7

The planet was discovered on the night of August 16-17.

Discovery and Elliptical Elements of Comet II. 1873.

This comet was discovered by M. Tempel, at Milan, on the night of July 3, its approximate position being R.A. = $1^h 51^m$ and N.P.D. = $94^\circ 34'$. The following elliptical elements have been computed by Dr. Bruhns from observations made at the Observatory of Leipzig:—

1873, June 25.530644 Mean Time Berlin.

$$\left. \begin{aligned} \pi &= 306^\circ 25' 12'' \\ \Omega &= 121^\circ 10' 06'' \\ i &= 12^\circ 51' 28'' \\ \phi &= 34^\circ 25' 48'' \\ \log a &= 0.491741 \\ \mu &= 649''2229 \end{aligned} \right\} \text{Mean eq. 1873}^\circ.$$

Discovery of Comets III., 1873, and IV., 1873.

The first of these comets was discovered at Marseilles, by M. Borelly, on August 21. At 15^h Mean Time at Marseilles, the approximate R.A. was $7^h 27^m$, and N.P.D. $51^\circ 15'$. Its motion was rapid towards the south, nearly a degree in 24^h .—The second comet was discovered at Paris, on August 23, by MM. Paul and Prosper Henry. At 11^h Mean Time at Paris, the approximate R.A. was $7^h 27^m$, and N.P.D. $30^\circ 30'$, with a rapid motion towards the east. This comet was round, with a central nucleus, and almost visible to the naked eye.

The following notes, by MM. Rayet and André, on the comet discovered by MM. Henry, are extracted from the *Comptes Rendus*, No. 9 (Sept. 1, 1873):—

“On the day of the discovery, the comet had a circular form, with a luminous condensation in the centre, the intensity of the light decreasing in a regular and continuous manner. Its diameter was about three or four minutes of arc.

“On the nights of August 26 and 27 the sky was very clear, and we were able to examine the comet with moderately high powers. Its diameter was about six minutes, and it had preserved its circular form with a very visible condensation of light in the centre; there was no trace of a nucleus,* or of successive envelopes. The appearance of the comet was similar to that of the cluster in the constellation *Hercules*, as seen through a telescope not sufficiently powerful to resolve the cluster into stars. The brightness of the central nucleus* was considered comparable to that of a star of the seventh magnitude.

* *Noyau* in the original in both instances. There seems, however, a slight contradiction in the two statements as they stand, but the observers evidently intended to remark that there was no trace of a *stellar* nucleus. [E. D.]

"The spectrum of the comet was composed of three ordinary luminous bands. The first in the yellow, about half way between D and E; the second near *b*; the third beyond F. There was no trace of a continuous spectrum between the luminous bands. The line in the green was much more brilliant, and appeared of double the length of the two others; it was clearly defined on the side of the red, but became diffuse on the side of the violet. The lines in the yellow and blue were nearly of equal intensity.

"On the night of August 29, the diameter of the comet was much increased. It spread out to nearly eight minutes of arc, having attached to it a rather broad tail of nearly 25' in length, directed in opposition to the Sun, and inclined about 47° in the direction of the diurnal movement. The head of the comet had preserved its roundness, and the brightness of the central nucleus had increased to that of a star of the sixth magnitude. The tail, less luminous near the nebulosity of the head, became brighter afterwards, and disappeared gradually at the extremity.

"The head of the comet always gave a spectrum composed of three luminous bands, but traversed on this occasion by a very faint continuous spectrum. Owing to the greater brightness of the comet, the spectrum observations could be made with a comparatively narrow slit, and the green band had therefore a clearer definition; in one portion of it, it terminated on both sides in a straight line, always, however, remaining more brilliant on the side of the red. The intensity of the other lines were also slightly increased."

The spectrum of the comet discovered by M. Borelly was examined by MM. Wolf and Rayet. They found it composed of a continuous spectrum from the yellow almost to the violet, due evidently to reflected solar light, and of two luminous bands, one in the green, and the other in the blue. The former was clearly defined towards the red, but diffused towards the violet; the blue band, which was about half as intense as the green, was also similarly defined.

MM. Wolf and Rayet remark that the continuous spectrum was of much greater intensity than that of comets previously examined by them. It was also more narrow, which, perhaps, may be due to a solid nucleus.

Note.—The Editor reminds Fellows that all papers and communications for the next Meeting should be sent to Mr. Dunkin, Hon. Sec.

Owing, probably, to the circumstance that Mr. Proctor's papers were in type earlier than the rest, in view of his departure from England, the printers have set those papers first in the paged matter also. It did not seem worth while to have the matter reset; but Mr. Proctor deems it necessary to indicate the cause of the unusual arrangement of the papers in the present number.

INDEX.

	Page
AIRY, Sir G. B., letter from, to the Secretary of the Admiralty on the transit of <i>Venus</i>	269
—, —, —, on the want of observations of <i>Jupiter's</i> satellites	368
Altazimuth, proposed double, Mr. Carrington	117
— diagram, new use of, Rev. Mr. Freeman	297
Application to Government for the organization of parties of observers in the Southern Seas for the transit of <i>Venus</i> in 1874	461
Arc of the meridian, measured in South Africa, Mr. Todhunter	27
Associates elected	65
Associates deceased, list of	190
M. Charles E. Delaunay	203
Prof. F. Kaiser	209
M. Paul A. E. Laugier	211
Prof. F. M. Schward	214
Astronomy, report on progress of, in the year 1872-73	232
Discovery of minor planets	232
Galvanic determinations of longitude	233
Munich zone-catalogues of stars	235
Gilliss' catalogue of 1963 stars	236
The right ascensions of equatorial fundamental stars	ib.
The masses of the planets and the solar parallax	237
Motions of stars in the line of sight	238
The evidence respecting a resisting medium in space	239
Flying shadows during solar eclipses	242
Photographs of the total solar eclipse of December, 1871	243
Polariscopic observations of the corona during the total solar eclipse of December, 1871	245
Observations made during the recent annular eclipse of the Sun	246
Singular phenomenon in the solar sierra	ib.
The solar prominences	247
Observations of the bright lines in the spectrum of the solar atmosphere	248
Connexion between weather changes and the solar-spot period	249
Telescopic observations of meteors	251
Progress of meteoric astronomy in 1872-73	252
Auwers, Prof., on the preparations for observing transit of <i>Venus</i>	43
Biela's Comet, letter on the discovery of, Mr. Pogson	116
—, —, —, on the rediscovery of, M. Klinkerfues	128
—, —, —, on the rediscovery of, Capt. Tupman	130, 317
—, —, —, Mr. Hind	320
Binary Star, α Centauri, Mr. Hind	54
—, ξ Ursæ Majoris, Rev. F. Secchi	64
—, —, —, Mr. Hind	101
—, —, —, Castor, Mr. Wilson	71
—, —, —, Mr. Hind	100
—, —, —, ξ Ursæ, ζ Cancri, and μ^2 Bootis, Mr. Wilson	412
—, —, —, elements of μ^2 Bootis, Mr. Wilson	460
—, —, —, ξ Ursæ, Mr. Erck	460
Browning, J., on a modified form of solar eye-piece	58
—, —, —, on an observing chair	57
—, —, —, on a large automatic spectroscope	410
—, —, —, on a spectrometer	411
—, —, —, note on the disappearance of the coloured equatorial belt of <i>Jupiter</i>	475
Bruhns, Dr., on Tempel's Comet of short period	327
Buffham, W., markings observed on <i>Uranus</i>	164

	Page
Burnham, S. W., catalogue of eighty-one double stars observed at Chicago	351
———, ———, second catalogue of double stars	437
———, ———, errors in Sir W. Herschel's catalogue of double stars	567
Burton, C. E., meteor swarm of November 27, 1872	138
———, ———, note on the appearance of <i>Jupiter's</i> fourth satellites in transit	472
Carrington, R., on the rate of a clock going in a partial vacuum	51
———, ———, on a proposed double altazimuth	117
Chacornac, on a volcanic appearance in the Sun	65
Chinese observations of solar spots, Mr. J. Williams	370
Chimmo, Capt., observations of the meteors of Nov. 27, at Nassau	408
Chair, observing, Mr. Browning	57
Christie, W. H. M., on a recording micrometer	484
———, ———, note on two telescopic meteors	497
Clock, on the rate of a, in a partial vacuum, Mr. Carrington	51
———, rate of in a partial vacuum, Rev. T. R. Robinson	121, 295
———, barometric errors of, compensation for, Mr. Denison	122, 394
———, barometric errors of, Mr. Webster	296
———, by Dr. Franklin, Mr. Lecky	478
———, sympathetic influence between, Mr. Ellis	480
Cluster of stars, Mr. Russell	66
Colours of the components of γ <i>Delphini</i>	103
Comets:—	
First of 1818 (Pons), Mr. Hind	50
Of the November meteors, early appearance of, Mr. Hind	48
Biela's, see Biela's Comet.	
Brorsen's, note on the approaching reappearance of, Mr. Hind	324
Tempel's, of short period (1867 II.) sweeping ephemerides of, Mr. Hind	325
———, ———, observations made at Greenwich	500
———, ———, Dr. Bruhns, and Mr. Hind	327
———, ———, note on rediscovery of, M. Stéphan	410
———, ———, ephemeris of, Mr. Hind	459, 498
———, ———, observations of, M. Stéphan	459
Discovery and elliptic elements of Comet II. 1873	581
——— of Comets III. and IV. 1873	ib.
Comparison of standard stars observed at the Radcliffe Observatory, Oxford, in 1869, with the places deduced from the <i>Tabulæ Reductionum</i> , Prof. Wolfers	358
Co-ordinates of stars within and near the Milky Way, Mr. Marth	1, 517
Crossley, E., meridian marks for transit instruments	409
Death of Mrs. Somerville	65
Denison, E. B., on a compensation for the barometric errors of clocks	122, 294
Denning, W. F., meteor-shower of November 27, 1872	93
———, visibility of <i>Jupiter</i>	179
Denza, meteor-shower of November 27, 1872	97
<i>Delphini</i> , γ . colours of, Mr. Elger	103
Double-stars, catalogue of 81 observed at Chicago, Mr. Burnham	351
———, geometrical investigation of orbits of, Mr. Wilson	375
———, second catalogue of new double-stars, Mr. Burnham	437
Dunkin, E., on an error in the R.A., of 3735 <i>Groombridge</i>	166
———, ———, on the <i>Nautical Almanac</i> values of the semi-diameters of the Sun and <i>Venus</i> , used in the calculations of the transit of <i>Venus</i>	293
———, ———, note on the discovery of minor planet (131)	531
Eclipses, future solar, Mr. Maguire	98
———, total solar, June 14, 2151, Mr. Hind	180
———, ———, ———, Mr. Maguire	469
———, mentioned in the Anglo-Saxon chronicles, Rev. S. J. Johnson	402

	Page
Eclipses, partial solar, May 25, 1873, Capt. W. Noble.....	468
—, —, —, —, —, —, Rev. S. J. Perry.....	469
—, —, —, —, —, —, Mr. Pratt.....	577
Elger, T. G. E., on the colours of γ <i>Delphini</i>	103
—, —, —, observations of the planet <i>Venus</i> in 1873	424
Ellery, R. L. J., on the preparation of speculum metal	579
Ellis, W., on sympathetic influence between clocks	480
Equatorial, apparatus for connecting hour-circle with regulator, Mr. Erck..	174
Errata	116, 180, 368, 460, 532
Erck, W., on an apparatus for connecting hour-circle of Equatorial with the regulator	174
—, —, position of components of ξ <i>Ursæ Majoris</i>	460
Fasel, V., meteor-shower of November 27, 1872	90
—, —, on the zodiacal light	99
Fellows, deceased, list of	189
Nathaniel Beardmore	197
Beujamin Godfrey, M.D.....	198
Richard Hodgson	199
Prof. Adam Sedgwick, LL.D., F.R.S	199
Fellows elected	1, 65, 117, 181, 269, 369, 413, 461
Fireball in Scotland, Mr. Penny	177
Firmstone, E., <i>Jupiter's</i> fourth satellite	460
Fleurien, appointment of, to observe transit of <i>Venus</i> at the Marquesas Islands	368
Flora, proposal to determine the solar parallax from observations of, Dr. Galle	492
—, comparison stars and ephemeris of, for the opposition of 1873, October and November.....	494
Forbes, Prof., on the meteor-shower of November, 1872	407
Freeman, Rev. A., graphic conversion of stellar co-ordinates	18
—, —, —, new use of altazimuth diagram.....	297
Galle, Dr., proposal to determine the solar parallax by observations of <i>Flora</i>	492
Gill, David, on Lord Lindsay's preparations for observations of the transit of <i>Venus</i> in 1874	34
Glaisher, J. W. L., on the progress to accuracy of logarithmic tables	330
—, —, —, on the rejection of discordant observations	391
—, —, —, on logarithmic tables	440
Grant, Prof., meteor-shower of November 27, 1872	78
Graphic conversion of stellar condinates, Rev. A. Freeman	18
Gray, Lieut. F. J., meteor-shower of November 27, 1872.....	178
Greenwich, Royal Observatory, occultations of stars.....	157
—, —, —, —, phenomena of <i>Jupiter's</i> satellites	157
Greg, Mr., errata in tables of meteor-showers	64
Groombridge 3735, on an error in the R.A. of, Mr. Dunkin	166
Hall, A., on the determinations of longitudes by Moon culminations	465
Herschel, Prof. A. S., meteor-showers of November 27	73
—, —, —, positions of the radiant point of the meteor-shower of November 27, 1872	501
Honorary member, deceased, Mary Somerville	190
Hind, J. R., on two probable early appearances of the comet of the November meteors	48
—, —, —, on the first comet of 1818	50
—, —, —, on the binary star α <i>Centauri</i>	54
—, —, —, on the radiant point of the meteors of November 27, 1872....	98
—, —, —, on the binary star α <i>Geminorum</i>	100
—, —, —, on the orbit of ξ <i>Ursæ Majoris</i>	101
—, —, —, transit of <i>Venus</i> , 1874, at Port Louis, Mauritius	115
—, —, —, total solar eclipse of 2151, June 14	189
—, —, —, on the state of calculations with respect to Biela's comet	320
—, —, —, on the reappearance of Brorsen's comet of short period	324

	Page
Hind, J. R., sweeping ephemerides for Tempel's comet of short period (1867, II)	325
—, —, note on Tempel's comet of short period.....	327
—, —, ephemeris of Tempel's comet	459, 498
Ingpen, John E., description of a prismatic solar-eyepiece	59
Janssen, M., Passage de <i>Venus</i> : méthode pour obtenir photographiquement l'instant des contacts.....	380
Johnson, Rev. S. J., on the eclipses mentioned in the Anglo-Saxon Chronicle	402
Jupiter, visibility of, Mr. Denning	179
—, in 1873, note on, Mr. Knobel	474
—, —, disappearance of coloured equatorial belt of, Mr. Browning	475
—, mass of, Mr. Lynn	475
Jupiter's satellites, phenomena of, observed at Greenwich	157
—, —, want of observations of, Sir G. B. Airy	368
—, —, Rev. S. J. Perry	426
—, —, observations of, at the Radcliffe Observatory.....	486
—, —, fourth satellite, observations of transit of, Mr. Roberts	412
—, —, on and off the disc, Mr. Firmstone	460
—, —, Mr. Burton	472
Klinkerfues, M., on the rediscovery of Biela's Comet	128
Knevitt, Capt., on a phenomenon observed at sea	411
Knobel, E. B., note on <i>Jupiter</i> , 1873	474
—, —, note on <i>Mars</i> , 1873	476
Konkoly, M., spectroscopic observations of meteors.....	575
Langdon, R., observations of <i>Venus</i>	500
Lecky, R. J., on a clock by Dr. Franklin	478
Lindsay, Lord, preparations for observing transit of <i>Venus</i> in 1874	34
Livingstone, Dr., reduction of geographical observations by, Sir T. Maclear	575
Logarithmic tables, progress to accuracy of, Mr. Glaisher	330
—, —, Mr. Glaisher	440
—, —, note on, Col. Tennant	563
Lohre, Dr., method for determining the heat radiation from the Sun-spots..	579
Longitudes, determinations of, by Moon culminations, Mr. Hall.....	465
—, —, —, lunar distances, Col. Tennant.....	565
Lowe, E. J., on the meteors of November, 1872	81
Lunar volcanoes, origin of, Mr. W. M. Williams	360
—, —, atmosphere, note on the possible existence of, Mr. Neison	464
Lynn, W. T., on the parallax, &c., of Lalande 21185	52
—, —, on the proper motion of Lalande 21258	102
—, —, —, Groombridge 1830	102
—, —, note on the mass of <i>Jupiter</i>	475
Maclear, Sir T., reduction of observations of geographical positions, by Dr. Livingstone.....	575
Maguire, J., on future solar eclipses	98
—, —, on the total solar eclipse of June 14, 2151	469
Main, Rev. M., observations of occultations and of the phenomena of <i>Jupiter's</i> satellites at the Radcliffe Observatory.....	486
Mars, rotation of, Mr. Proctor	552
—, note on projections of, Mr. Proctor	160
Marth, A., co-ordinates of stars within and near the Milky Way	1, 517
—, —, ephemeris for observations of the Moon	139, 365
—, —, ephemeris of the five inner satellites of <i>Saturn</i>	513
Meldrum, C., star-shower seen at Mauritius, Nov. 27	131
Meridian marks for transit instruments, Mr. Crossley	409
—, —, Capt. Noble	527
Meteor observed at Mauritius, Mr. Wright	176

	Page
Meteors, observations of, Capt. Tupman.....	298
———, radiant points, catalogue of, &c., Capt. Tupman	300
———, general horary numbers, Capt. Tupman.....	308
———, note on two telescopic, Mr. Christie	497
———, Spectroscopic observations of, M. Konkoly	575
Meteor-shower of November 27, 1872:—	
Prof. Herschel	73
Prof. Grant.....	78
Mr. Lowe	81
Lord Rosse.....	89
M. Fasel.....	90
Rev. S. J. Perry.....	92
Mr. Denning	93
M. Pihl	95
M. Denza	97
Mr. Wilson.....	97
Miss Readhouse.....	97
Mr. Hind	98
Mr. Meldrum	131
Prof. Herschel	136, 501
Com. Wharton	138
Mr. Burton.....	138
Lieut. Gray	178
Prof. Forbes	407
Capt. Chimmo	408
Meteor-showers, errata in tables of, Mr. Greg	64
Minor planet. <i>See</i> Planets, Minor.	
Moon, ephemeris for physical observations of, Mr. Marth	139, 365
Moon's disk, apparent projection of stars on, Mr. Plummer	345
———, ———, Mr. Proctor	349
Mummary, Rev. J. V., presents to the Society a photograph of Mrs. Somerville	461
Nebular regions in <i>Virgo</i> and <i>Coma Berenices</i> , Mr. Proctor	14
November meteors, origin of, Mr. Proctor.....	45
Nebulæ, note on the distribution of resolvable and irresolvable, Mr. Waters	406
———, new, observed at Marseilles, M. Stéphan	433
——— and clusters, distribution of, Mr. Waters	558
Neison, E., note on the possible existence of a lunar atmosphere	464
Newcomb, Prof., on a mechanical representation of a familiar problem	573
——— some cases in the method of least squares	574
Noble, Capt. W., observations of the partial solar eclipse of May 25, 1873..	468
———, ———, note on Mr. Crossley's paper on "Meridian Marks for Transit Instruments"	527
Norton, Prof., reclamation on behalf of, Mr. Proctor	536
Object-glasses, diffraction of, Hon. J. W. Strutt	59
Occultations of stars, observed at Greenwich	157
———, Radcliffe Observatory	486
Observatories, accounts and proceedings of:—	
Royal Observatory, Greenwich.....	215
Radcliffe Observatory, Oxford.....	217
Cambridge Observatory.....	218
Royal Observatory, Edinburgh.....	220
Dunsink (Dublin) Observatory	221
Glasgow Observatory.....	222
Liverpool Observatory	ib.
Durham Observatory.....	223
Kew Observatory	224
Stonyhurst Observatory.....	225
Rugby Observatory	ib.

	Page
Observatories, accounts and proceedings of:—	
Lord Lindsay's Observatory.....	226
Mr. Barclay's Observatory	227
Mr. Buckingham's Observatory	ib.
Mr. Knott's Observatory	228
Royal Observatory, Cape of Good Hope.....	ib.
Melbourne Observatory.....	229
Sydney Observatory	231
Observations, rejection of discordant, Mr. Glaisher	391
Oppolzer, Prof., elements of the minor planet <i>Peitho</i> (1118).....	179
Penny, H. D., fireball in Scotland	177
Penrose, F. C., graphical representation of the circumstances of the transit of <i>Venus</i> in 1874	382
Perry, S. J., on the meteors of November 27, 1872	92
—, —, phenomena of <i>Jupiter's</i> satellites	426
—, —, on a self-recording transit micrometer	434
Peters, Dr., discovery of minor planet (129).....	368
Photographs taken during the total solar eclipse of December 11–12, 1871. Lieut.-Col. Tennant.....	23, 467
Pihl, M., on the meteors of November 27, 1872	93
Planet, minor, (128), Mr. Watson	116
—, —, (118), elements of, Prof. Oppolzer.....	179
—, —, (129), Dr. Peters	368
—, —, (131), note on the discovery of, Mr. Dunkin.....	531
—, —, (133), discovery of, Prof. Watson	580
Plummer, J. I., on the apparent projection of stars on the Moon's disk	345, 417
—, —, note on the figure and diameter of <i>Venus</i>	422
—, —, measures of the diameter of <i>Venus</i>	
Pogson, N. R., letter on the discovery of Biela's comet	116
Pratt, Henry, observations of a portion of the Moon's limb during the late solar eclipse.....	
Proctor, R. A., the rich nebular regions in <i>Virgo</i> and <i>Coma</i>	14
—, —, on the origin of the November meteors	45
—, —, note accompanying charts of stellar proper motions	105
—, —, note on projections of <i>Mars</i>	160
—, —, remarks on a letter from Sir G. B. Airy respecting the transit of <i>Venus</i>	273
—, —, table showing differences of duration of the transit of <i>Venus</i> in 1874 at thirty-one stations	278
—, —, the transit of <i>Venus</i> in 1874	279
—, —, on the direct determination of the parallactic displacement of <i>Venus</i> during her transit	291
—, —, note on Mr. Plummer's paper on the projection of stars on the Moon's disc	349
—, —, note on the approaching transit, with special reference to the probability of absolute failure through the fewness of southern stations.....	381
—, —, graphical method for determining the motion of a body in an elliptic orbit under gravity.....	386
—, —, notes on star-gauging	461
—, —, note addressed to the astronomers of the United States on the subject of the approaching transit of <i>Venus</i>	413
—, —, note on Mr. Plummer's reply respecting the projection of stars on the Moon's disk.....	417
—, —, note explanatory of a stereographic chart of the transit of <i>Venus</i> in 1874.....	421
—, —, note explanatory of a stereographic chart of the transit of <i>Venus</i> in 1882.....	470
—, —, on a new method of observing transits of <i>Venus</i>	530
—, —, note accompanying a chart of southern regions suitable for observing the transit of <i>Venus</i> in 1874	533

	Page
Proctor, R. A., further notes on star-gauging	535
———, ———, on the expulsion theory of the solar corona, comets, &c.....	536
———, ———, statement of views respecting the sidereal universe	539
———, ———, on the rotation-period of <i>Mars</i>	552
Procyon, observations of, as a double star, Mr. O. Struve	430
Proper motion of Lalande 21185, Mr. Lynn	52
——— motions. note accompanying charts of, Mr. Proctor.....	105
Prominences, solar, observations of, Capt. Tupman	105
——— Lalande 21258, Lynn.....	102
——— Groombridge 1830, Mr. Lynn	102
Rabbins, views of the ancient. respecting the dimensions of the Earth, Mr. Wackerbarth ..	576
Readhouse, Miss, on the meteors of November 27, 1872	97
Roberts, G. W., observation of transit of <i>Jupiter's</i> fourth satellite	412
Robinson, Rev. T. R., on the rate of a clock going in a partial vacuum 121, 295	
Rosse, Lord, on the meteors of November 27, 1872	89
Rotation of <i>Mars</i> , Mr. Proctor.....	552
Russell, H. C., search for <i>Vulcan</i>	437
———, ———, on the coloured cluster of stars about α <i>Crucis</i>	66
Saturn, ephemeris of five inner satellites of, Mr. Marth	513
Secchi, Rev. F., measures of ξ <i>Ursæ Majoris</i>	64
Shooting-stars, results of observations of, Capt. G. L. Tupman	298
Sidereal universe, statement of views respecting, Mr. Proctor.....	539
Slatter, Rev. J., on an instance of abnormal refraction.....	364
Semi-diameters of the Sun and <i>Venus</i> , Mr. Dunkin	293
Society:—	
Report of Council to the fifty-third annual general meeting.....	181
Government aid to the cultivation of physics.....	189
List of papers read from February 1872 to February 1873	261
Library, list of donors to	265
Officers and Council for 1873-74.....	268
Solar eyepiece, modified form of, Mr. Browning	58
———, description of a prismatic	59
Somerville, Mrs., death of	65
Southern stars, observations of the colours and magnitudes of, Capt. Tupman	317
Spectroscope, on a large automatic, Mr. Browning	410
Spectroscopic observations of meteors, by M. Konkoly.....	575
Spectrometer, Mr. Browning	411
Speculum metal, on the preparation of, Mr. Ellery	579
Star-gauging, notes on, Mr. Proctor	461, 535
Stars, mean places of 78, near the South Pole, Mr. Stone	55
Stéphan, M., note on the re-discovery of Tempel's comet of short period ..	410
———, —, new nebulae discovered at the Observatory of Marseilles	433
———, —, observations of Tempel's Comet, 1867, II.	459
Stone, E. J., mean places of 78 southern stars	55
———, —, discordance in determinations with transit-circle	169
———, —, on the most probable result derivable from a number of direct determinations of equal value	570
Strutt, J. W., on the diffraction of object-glasses	59
Struve, M. Otto, list of stations for observation of the transit of <i>Venus</i> by Russian astronomers	415
———, ———, observations of <i>Procyon</i> as a double-star	430
Sun, volcanic appearance in, M. Chacornac	63
Sun spots, method of determining heat-radiation from, Dr. Lohre	579
Tennant, Lieut. Col. J. F., on the photographs taken during the total solar eclipse of December 11-12, 1871	23, 467
———, ———, note on observing lunar zenith-distances for longitude.....	565
———, ———, note on logarithmic tables	563

	Page
Todhunter, J., on the arc of the meridian measured in South Africa	27
Transit of <i>Venus</i> in 1874, Lord Lindsay's preparations for, Lord Lindsay and Mr. Gill	34
———, preparations for observing, M. Auwers	43
———, circumstances at Port Louis, Mauritius, Mr. Hind	115
———, Sir G. B. Airy	269
———, Mr. Proctor. . 273, 278, 279, 291, 381, 413, 421, ...	533
———, American preparations for observing	378
———, photographic observations of, M. Janssen	380
———, Mr. Penrose	382
———, list of stations for observing by Russian astronomers, M. Struve.....	415
———, application to Government for organisation of observing parties	461
——— 1882, note on stereographic chart of, Mr. Proctor	470
———, on a new method of observing, Mr. Proctor	530
Transit-circle (Cape of Good Hope) discordance between readings for zenith point, Mr. Stone.....	169
Transatlantic cables, transmission of free messages on astronomical subjects by	369
Transit micrometer, on a self-recording, Rev. S. J. Perry	434
———, ———, Mr. Christie	484
Tupman, Capt. G. L., observations of the solar prominences	105
———, ———, on the rediscovery of Biela's comet	130, 317
———, ———, results of observations of shooting-stars, made in the Mediterranean	298
———, ———, colours and magnitudes of southern stars	312
Uranus, markings on, Mr. Buffham	164
Venus, transit of, see transit of <i>Venus</i>	
———, markings on, Mr. Wilson.....	294
———, note on the figure and diameter of <i>Venus</i> , Mr. Plummer.....	422
———, observations of, in 1873, Mr. Elger	424
———, ———, Mr. Langdon	500
———, measures of the diameter of, Mr. Plummer	559
Volcanoes, lunar, origin of, Mr. W. M. Williams	360
Vulcan, search for, Mr. Russell	437
Wackerbarth, A. D., views of the ancient Rabbins relative to the dimensions of the Earth	576
Waters, Sidney, note on the distribution of resolvable and irresolvable nebulae.....	406
———, ———, the distribution of clusters and nebulae	558
Watson, Prof., discovery of minor planet (128)	116
———, ———, (133)	580
Webster, R., on the barometric error of clocks	296
Wharton, Com. W. J. S., star-shower at Malta, November 27	138
Williams, W. M., on the origin of lunar volcanoes	360
Williams, J., Chinese observations of solar spots	370
Wilson, J. M., note with chart of stars of <i>Castor</i>	71
———, ———, on the meteors of November 27, 1872	97
———, ———, geometrical investigation of the orbit of a double-star	375
———, ———, on the markings of <i>Venus</i>	294
———, ———, recent measures of ξ <i>Ursæ</i> , ξ <i>Canceri</i> , and μ_2 <i>Boötis</i>	412
———, ———, elements of orbit of μ_2 <i>Boötis</i>	460
Wolfers, Prof., comparison of places of standard stars as observed at Oxford, and as deduced from the <i>Tabulæ Reductionum</i>	358
Wright, M., meteor observed at Mauritius.....	176
Zodiacal Light, Mr. Fasel.....	99

LIST OF PRESENTS

RECEIVED DURING THE SESSION OF 1872-73,

FORMING

APPENDIX XXIV.

To the Catalogue of the Library of the Royal Astronomical Society.

- | | |
|---|--|
| Abbatt, R., Elements of Physical Astronomy, 8vo. | The Author. |
| <i>London, 1872</i> | |
| Abbott, F., Results of Five Years' Meteorological Observations
for Hobart Town, in which are incorporated the Results
of Twenty-five Years' Observations previously published
by the Royal Society of Tasmania, folio. | ——— |
| <i>Hobart Town, 1872</i> | |
| Abetti, Dr. A., Cenni storice sur successivo suiluppo della
Meteorologia e su alcune sue importanti applicazione.
8vo. | ——— |
| <i>Padova, 1872</i> | |
| Académie des Sciences de l'Institut de France, Comptes
Rendus, t. lxxiv. No. 22, to t. lxxvi. No. 21, 4to. | The Academy
of Sciences. |
| <i>Paris, 1872-73</i> | |
| Actuaries, Institute of, Tables deduced from the Mortality
experience of Life Assurance Companies, collected and
arranged by the, 8vo. | The
Institute. |
| <i>London, 1872</i> | |
| American, The, Ephemeris and Nautical Almanac for 1875,
8vo. | The U. S. A.
Bureau of
Navigation. |
| <i>Washington, 1872</i> | |

- The Author. Anderson, F., Bestamung af Planeten (86) Semeles Bana, 4to,
Lund, 1872
- The Society. Asiatic Society of Bengal, Journal, No. 173 to 175.
Calcutta, 1872
- ————, Proceedings, 1872, June to August,
8vo. *Calcutta*, 1872
- The Author. Astrand, J, J., Ny Interpolations-methode, 8vo.
Christiania, 1871
- Barbera, Luigi, Saggi di Filosofia Naturali, 8vo.
Napoli, 1873
- Bazerque, Captain, La Caravane Universelle, Voyages autour
du Monde, 8vo. *Paris, n. y.*
- The Academy. Berlin Royal Academy, Abhandlungen, 1871, 4to.
Berlin, 1872
- ————, Monatsbericht, March to Nov. 1872,
Jan. 1873, 8vo. *Berlin*, 1872-73
- The Imperial Observatory, Berlin, Astronomisches Jahrbuch fur 1875, 8vo. *Berlin*, 1873
- The Society. ——— Physical Society, Der Fortschritte der Physik im Jahre
1868, Pts. 1, 2, 8vo. *Berlin*, 1872
- ————, Namen und Sach. Register der Do.
Band 1, bis xx., 8vo. *Berlin*, 1872
- The Author. Bethune, Admiral, Tables for Travellers adapted for the
Pocket or Sextant-case, 12mo. *London*, 1872
- The Royal Institute of Bologna, Memorie dell' Accademia delle Scienza dell' Istituto
di, t. ix., pts. 1, 2, 3, and 4, 4to. *Bologna*, 1869-70
- ————, Rendiconto, 1869-70, 8vo. *Bologna*, 1869-70
- The Bologna Academy. ——— Academy of Sciences, Memorie, Serie 3, fasc. 1 to 4,
4to. *Bologna*, 1872
- ————, Indici Generali die dieci Tomi
della, secondo serie, 8vo. *Bologna*, 1871
- ————, Rendiconto, 1871-1872, 8vo.
Bologna, 1872

- Bonomi, J., Project of an Instrument for the identification of The Author.
Persons, 8vo. *London, 1872*
- Bombay, Magnetical and Meteorological Observations made H.M. Secretary
at the Government Observatory from 1865 to 1870, 4to. of State for
India.
Bombay, 1872
- Bordeaux Société des Sciences, Mémoires, t. 1, 2 pt., i. to viii., The Society.
8vo. *Bordeaux, v.y.*
- Bothkamp Observatory, Beobachtungen angestellt auf der M. von Bülow.
Sternwarte des Kammerherrn von Bülow, heft. 1, 4to.
Leipzig, 1872
- Bruhns, C. und Weiss, E., Bestimmung der Langen differenz The Authors.
zwischen Leipzig und Wein auf Telegraphischen Wege,
8vo. *Leipzig, 1872*
- Bruhns, C., Mittheilung über die Ermittlung der Co-ordinaten —
der Plirssenburg und verschiedener Thürme in Bezug auf
die Leipziger Sternwarte und über die construction eines
Basis Apparatus, 8vo. *Leipzig, 1872*
- Bruxelles, l'Observatoire Royal de, Annales, t. xxi., 4to. The Royal
Observatory.
Bruxelles, 1872
- , Annuaire, 1872, 1873, —
12mo. *Bruxelles, 1872-73*
- Bruxelles, Académie Royale des Sciences, Mémoires, t. xxxix., The Academy.
4to. *Bruxelles, 1872*
- , Mémoires Couronnés, —
t. xxii, 8vo. *Bruxelles, 1872*
- , Bulletins, 1871-72, —
8vo. *Bruxelles, 1871-72*
- , Centième Anniver- —
saire de Fondation de, 8vo. *Bruxelles, 1872*
- , Annuaire, 1872-73, —
12mo. *Bruxelles, 1872-73*

- S. M. Drach, Buffier, P., *Geografia universale esposta nei differenti metodi che passono abbreviare lo studio e facilitar l'uso di questa Scienza col soccorso di Versi artificiale*, 12mo.
Venezia, 1752
- The Royal Observatory. Cape of Good Hope Royal Observatory, Results of Astronomical Observations in the years 1856-57, under the superintendence of Sir Thomas Maclear, reduced and printed under the superintendence of E. J. Stone, Esq., M.A., 8vo.
Cape Town, 1871-72
- E. J. Stone, Esq. ————— Proceedings of the Forty-third Anniversary Meeting of the Subscribers to the South African Public Library, Cape Town, held on Saturday, May 18, 1872, 8vo.
Cape Town, 1872
- The Society. Cherbourg Société Nationale des Sciences Naturelles, Mémoires, t. xvi., 8vo.
Paris, 1871-72
- The University. Christiania University, Norske Meteorologisk Aarbog for 1871, ob. folio
Christiania, 1872
- The Observatory. Coimbra, Ephemerides Astronomicas, 1874, 8vo,
Coimbra, 1872
- D'Abbadie, Antoine, Géodésie d'Ethiopie, ou Triangulation d'une partie de la Haute Ethiopie, 4to.
Paris, 1873
- The Society. Denmark, Oversigt over det Kongelige, Danske videnskabernes Selskabs Forhandlingar og dets Medlemmers Arbejder, No. 3, 1871, No. 1, 1872, 8vo.
Kjobenhavn, 1871-72
- The Author. Denning, W. F., Astronomical Phenomena in 1872, 8vo.
London, 1872
- The Dépôt Gen. de la Marine. Dépôt Gen. de la Marine, Books presented by :—
Annales Hydrographiques, Nos. 492, 497, 498, 503, 8vo.
Paris, 1871-72
- Renseignements des Iles Havaii ou Sandwich, par A. Frickmann, 8vo.
Paris, 1871
- Routier de l'Australie, 1^{re} partie, par M. Frickmann, 8vo.
Paris, 1871

Depôt General de la Marine, Books presented by:—		The Depôt Gen. de la Marine.
Côte Occidentale de l'Amérique du Nord, Nos. 501, 502, par A. Frickmann, 8vo.	<i>Paris, 1872</i>	—
Recherches sur les Chronomètres, 9 ^{me} cahier, 8vo.	<i>Paris, 1872</i>	—
Etudes des Embouchures du Nil, par M. La Rousse, 8vo.	<i>Paris, 1872</i>	—
Instructions Nautiques pour la Côte d'Irlande, 1 ^{re} partie 8vo.	<i>Paris, 1871</i>	—
——— Nautiques sur la Côte du Pérou, par F. Char- donneau, 8vo.	<i>Paris, 1872</i>	—
——— pour Naviguer dans le Detroit de Magellan, 8vo.	<i>Paris, 1872</i>	—
Annuaire des Marées des Côtes de France, 1873, 12mo.	<i>Paris, 1872</i>	—
Donati, G. B., Del resto di una Cometa con la Terra, e partico- larmente della Cometa di Biela, 8vo.	<i>Florence, 1872</i>	The Author.
———, Di alcuni Fenomeni che si manifestarono sulle Linee Telegrafiche durante la grande Aurora Boreale dal 4 Febbraio, 1872, 4to.	<i>Firenze, 1873</i>	—
Drach, S. M., An easy General Rule for filling up all Magic Squares, 8vo.	<i>London, 1873</i>	—
———, Observations on the Base length of the Great Pyramid and Royal Coffin dimensions, 8vo.	<i>London, 1873</i>	—
Draper, Dr., Researches in Actino Chemistry, 8vo.	<i>Newhaven, U. S., 1872</i>	—
Dudley Observatory, U. S. A., Annals, vol. ii., 8vo.	<i>Albany, 1871</i>	The Observa- tory.
Edinburgh Royal Society, Transactions, vol. xxvi., pt. 4, 4to.	<i>Edinburgh, 1873</i>	The Society.
———, Proceedings, 1871-72, 8vo.	<i>Edinburgh, 1873</i>	—

- The American Academy. Ellis, G. E., Memoir of Sir B. Thompson, Count Rumford, with Notices of his Daughter, 8vo. *Philadelphia, n. d.*
- The Author. Erienmeyer, Dr. E., Die aufgabe des Chemischen Unterrichts gegenuber den anforderungen der Wissenschaft und Technik radi, 4to. *Munchen, 1871*
- Faye, M., Sur la Situation actuelle du Bureau des Longitudes, 4to. *Paris, 1872*
- Fergola, E., Determinazione novella della Latitudine del R. Osservatorio di Capodemonte mediante le differenze di distanze Zenitale meridiani de 52 Coppie di stelle osservate durante l'anno 1871, 4to. *Napoli, 1873*
- Gautier, A., Review of Prof. Smyth's Pamphlet of "Extracts from the 13th vol. of the Edinburgh Observations" (in French), 8vo. *Génève, 1872*
- ———, Review of A. Buchan's "Remarks on the Climate of Jerusalem" (French), 8vo. *Génève, 1872*
- ———, Review of "Détermination Télégraphique de la difference de Longitude entre Righi Kulm et Zurich et Neuchatel, par Plantamour, Wolf, et Hirsch," 8vo. *Génève, 1872*
- ———, Review of "Rapport Annuelle du President de la Société de Génève, de Juin 1871 à Juin 1872" (French), 4to. *Génève, 1872*
- ———, Review of "Compte Rendu de la Conférence Géodésique internationale réunie à Vienne, du 21 au 28 Sept., 1871," 8vo. *Génève, 1872*
- The Society. Geneva Société de Physique, Mémoires, t. xxi., pt. 2. 4to. *Paris and Basle, 1872*
- Göttingen Royal Society, Abhandlungen, vol. xvii., 4to. *Göttingen, 1872*
- ———, Gelehrte Anzeigen, 1872, 8vo. *Göttingen, 1872*
- ———, Nachrichten, 1872, 8vo. *Göttingen, 1872*

- Greenwich Royal Observatory, Report of the Astronomer Royal to the Board of Visitors, 1873, June 7, 4to. *London*, 1873 The Astronomer Royal.
- Hall, Asaph, and Harkness, Wm. Reports on Observations of Encke's Comet during its return in 1871, 4to. *Washington*, 1872 The U. S. Naval Observatory.
- Hankel, W. G., *Elettrische untersuchungen, Abhandlungen* 29, 8vo. *Leipzig*, 1872 The Author.
- Heis, Ed., *Atlas cœlestis novus stellæ per mediam Europam solis oculis conspicuæ secundum veras Lucis magnitudines e cœlo ipso descriptæ cum catalogo stellarum. Atlas, Catalogus*, 8vo. *Coeln*, 1872 The Author.
- Heis, Prof., *Wochenschrift fur Astronomie Meteorologie und Geographie*, Nos. 50 to 52, 8vo. *Munster*, 1872 —
- Hernstein, Karl, *Über die Einfluss der Electricitat der Sonne auf den Barometer Stand*, 8vo. *Wein*, 1872 —
- India Trigonometrical Survey. Extracts from the General Report on the Operations of the Great Trigonometrical Survey, 1871-72, folio. *Dehra Dun*, 1872 H. M. Government for India.
- Italian Spectroscopic Society. *Memorie*, v. n. 1872-73, 4to. *Palermo*, 1872-73 The Society.
- Journals. The *Athenæum Journal*, Nos. 2328 to 2379, 4to. *London*, 1872-73 The Editor.
- The Royal Asiatic Society, vol. vi., part 2, 8vo. *London*, 1873 The Society.
- The *Astronomical Register*, Nos. 115 to 176, 8vo. *London*, 1872-73 The Editor.
- *American Journal of Science and Arts*, Nos. 19 to 28, 8vo. *Newhaven (U. S.)*, 1872-73 The Editors.
- The *Astronomische Nachrichten*, Nos. 1890 to 1942, 4to. *Altona and Kiel*, 1872-73 Dr. Peters.
- *Actuaries, Institute of, Journal*, No. 88 to vol. xvii., part 5, 8vo. *London*, 1872-73 The Institute.

- The Editor. Journals. Bulletin des Sciences Mathématiques et Astronomiques, May 1872 to June 1873, 8vo. *Paris*, 1872-73
- The Institute. ——— The Canadian Journal, July to December 1872, 8vo. *Toronto*, 1872
- The Society. ——— Dublin Royal Society. Journal, vol. vi., No. 2, 8vo. *Dublin*, 1872
- The Association. ——— East India Association Journal, No. 3, 8vo. *London*, 1872
- The English Mechanic, Nos. 378 to 428. *London*, 1871-72
- The Institute. ——— Franklin Institute, Journal, Nos. 557 to 568, 8vo. *Philadelphia*, 1872
- Geological Society. ——— Geological Society, Quarterly Journal, Nos. 111 to 114, 8vo. *London*, 1872-73
- The Society. ——— Geographical Society, Royal, Journal, vol. xli. ———, Proceedings, vol. xvi., No. 2, 8vo. *London*, 1872-73
- J. Herapath, Esq. ——— Herapath's Railway Journal, Nos. 1723 to 1774, 4to. *London*, 1872-73
- The Horological Institute. ——— The Horological Journal, Nos. 166 to 176, 8vo. *London*, 1872
- The Institution. ——— London Institution, Journal, Nos. 15 to 20, 8vo. *London*, 1872-73
- The Society. ——— Meteorological Society, Quarterly Journal of the, vol. i. Nos. 3 to 6, 8vo. *London*, n. s. 1872-73
- The Society. ——— Meteorological Society, Proceedings of the, No. 5, 8vo. *London*, 1873
- The Editor. ——— Nature, Nos. 138 to 188, 4to. *London*, 1872-73
- Dr. Francis. ——— The Philosophical Magazine, Nos. 289 to 302, 8vo. *London*, 1872-73
- The Society. ——— The Photographic Journal, Nos. 235 to 241, 8vo. *London*, 1872

- | | | | |
|--|---|---------------------------|----------------------------------|
| Journals. | The Quarterly Journal of Science, Nos. 35 to 38, 8vo. | <i>London, 1872-73</i> | The Editor. |
| ———— | La Revue Scientifique, June 1872 to June 1873, 8vo. | <i>Paris, 1872-73</i> | ———— |
| ———— | Society of Arts, Journal, Nos. 1023 to 1072, 8vo. | <i>London, 1872-73</i> | The Society of Arts. |
| ———— | The Telegraphic Journal, Nos. 3 to 7. | | |
| ———— | Telegraph Engineers, The Society of, Journal, Nos. 1 to 7, 8vo. | <i>London, 1872-73</i> | The Society. |
| ———— | United Service Institution. Royal, Journal, Nos. 67 to 71, 8vo. | <i>London, 1872-73</i> | The Institution. |
| Kasan University, | Reports and Memoirs, in Russian, 1869-72, 8vo. | <i>Kasan, 1871-72</i> | The University. |
| Kew Observatory, | Report of the Committee for the 15 Months ending Oct. 21, 1872, 8vo. | <i>London, 1872</i> | The Kew Committee. |
| Krueger, A., | Untersuchung uber die Bahn des planeten Themis nebst einer neuen Bestimmung der anzeihung des Jupiter, 4to. | <i>Helsingfors, 1873</i> | The Author. |
| Laughton, J. K., | An Introduction to the Practical and Theoretical Study of Nautical Surveying, 8vo. | <i>London, 1872</i> | ———— |
| Le Clerc, Sebastian, | Pratica de Geometria in Carta e in Campo. Translated into Italian by Emerico Brulon, 12mo. | <i>Roma, 1746</i> | S. M. Drach, Esq. |
| Leeds, Literary and Philosophical Society, | Annual Report for 1871-72, 8vo. | <i>Leeds, 1872</i> | The Society. |
| Leiden Observatory, | Annalen, vol. iii., 4to. | <i>Haag, 1872</i> | The Observatory. |
| ———— | ————, Verslag van der Staat der 1871-72, 8vo. | <i>Amsterdam, 1871-72</i> | ———— |
| Leipzig, Astronomischen Gesellschaft, | Vierteljahrsschrift, 1872 und Jan. 1873, 8vo. | <i>Leipsig, 1872-73</i> | The German Astronomical Society. |
| Lincei, Accademia de, | Atti delle reale, tome xxv., anno 25, 4to. | <i>Roma, 1872</i> | The Academy. |

- The Author. Littrow, C. von, Bericht uber von der Herren, Dr. Bruhns, Dr. W. Forster, und Prof. E. Weiss ausgefuhrten Bestimmungen der Meridian differenzen Berlin, Wein, Leipzig, 4to. *Wein, 1872*
- The Society. Liverpool, Proceedings of the Literary and Phil. Society of, 1870-71, No. 26, 1872, 8vo. *Liverpool, 1872-73*
- J. Hartnup, Esq. ———, Report of the Astronomer to the Marine Committee Mersey Docks, and Harbour Board, for the year 1871, 8vo. *Liverpool, 1872*
- The Author. Listing, J. B., Ueber unsere jetzige Kenntniss der Gestalt und grosse der Erde, 12mo. *Gottingen, 1872*
- The Lombardic Institute. Lombardo, Instituto Reale, Memorie, vol. xii., fasc. 2, 3, 4, 4to. *Milano, 1871-72*
- , Rendiconto, v. n. 8vo. *Milano, 1871-72*
- The Author. Macdonald, J., The Origin and the Density of Comets, or a change in the Ecliptic of the Earth, being a concise treatise on the Natural Economy and the Transmutable order of the Planetary System, 8vo. *Prahran, 1869*
- The Author. Mailly, E., De l'Astronomie dans l'Académie Roy. de Belgique Rapport seculaire 1772-1872, 8vo. *Bruxelles, 1872*
- , Tableau d'Astronomie dans l'Hemisphère austral et dans l'Inde, 8vo. *Bruxelles, 1872*
- The Society. Manchester Literary and Philosophical Society, Proceedings, v. n., 8vo. *Manchester, 1872*
- The Council of Manchester. ——— Public Free Libraries, Twentieth Annual Report, 1871-72, 8vo. *Manchester, 1872*
- The Author. Mann, W. W., A New Decimal Metrical System founded on the Earth's Polar Diameter, and designed for adoption by all civilised nations as one common system, 8vo. *New York, 1872*
- Maury, M. F., Address before the National Agricultural Congress at its Meeting at St. Louis, May, 1872, 8vo. *St. Louis, 1872*

- | | | |
|--|------------------------|-------------------------------|
| Meteorological Conference, Report of the Proceedings at Leipzig, 8vo. | <i>London, 1873</i> | The Meteorological Office. |
| Meteorological Office, Quarterly Weather Reports, Jan. 1871 to June 1872, folio. | <i>London, 1872</i> | The Meteorological Committee. |
| —————, Discussion of the Meteorology of the part of the Atlantic lying north of 30° N., for the eleven days ending 5th February, 1870, by means of Synoptic Charts, Diagrams, and extracts from Logs, with remarks and conclusions, with a Vol. of Charts and Diagrams, folio. | <i>London, 1872</i> | ————— |
| Meyer, Arnold, Zur Theorie der unbestimmten ternären quadratischen Formen, 8vo. | <i>Zurich, 1871</i> | The Author. |
| Moller, Axel, Planet och Komet Observationer anstallda på Lunds Observatorium, 4to. | <i>Lund, 1872</i> | ————— |
| —————, Bericht über die Resultate einer neuen Berechnung der Elemente des Fayischen Cometen, 8vo. | <i>Leipsig, 1872</i> | ————— |
| —————, Elementer for Faye's Comet och Ephemerid för des Åhrkomst 1873, 8vo. | <i>Stockholm, 1872</i> | ————— |
| —————, Planet Observationer på Lunds Observatorium 1872, 4to. | <i>Lund, 1873</i> | ————— |
| —————, Om Faye'ska Kometens stora störning av 1841, 8vo. | <i>Lund, 1873</i> | ————— |
| Moncalieri, Bulletino Meteorologico, 1871 to Feb. 1873, folio. | <i>Torino, 1871</i> | The Collegio Carlo Alberto. |
| Moritz, A., Materialien zu einer Klimatologie des Kaukasus, abth. iii., bd. 1, 8vo. | <i>Tiflis, 1871</i> | The Author. |
| —————, Schemacha und seine Erbeben, 8vo. | <i>Tiflis, 1872</i> | ————— |

- The Observa-
tory. Munich Royal Observatory, Verzeichniss von 4093 telesco-
pischen Sternen zwischen -90° und -15° . Declination
welche in den Munchener Zonen Beobachtungen Vorkom-
men reducirt auf den Anfang des Jahres 1850, 8vo.
Munchen, 1872
- The Munich
Royal
Academy. ——— K. B. Akademie der Wissenschaften Abhandlungen,
vol. xi., part 1, 4to. *Munchen, 1871*
- ——— Sitzungsberichtes 1871, heft iii., 1872, heft i., ii., 8vo,
Munchen, 1871-72
- ——— Inhaltverzeichniss zu Jahrgang, 1860, 1870, 8vo.
Munchen, 1872
- The U. S.
Government Newcomb, Simon, Right Ascensions of Equatorial Funda-
mental Stars, 4to. *Washington, 1872*
- The Naples
Royal
Society. Naples, Royal Society, Atti, vols. iii., iv., 4to.
Napoli, 1866-69
- ———, Rendiconto, 1867-69, 8vo.
Napoli, 1867-69
- The Lords
Comm. of the
Admiralty. Nautical Almanac for 1876, 8vo. *London, 1872*
- The Society. Orleans County, Society of Natural Sciences, Archives of
Science and Transactions, vol. i., Nos. 4, 5, 8vo.
Newport, U. S., 1871-2
- The Author. Orsted, A. S., Bidrag til Kundskab om Egefamilien i Fortig
af nuled, 4to. *Kjobenhavn, 1871*
- The Italian
Eclipse
Commission. Palermo, Rapporti sulle Osservazioni del Ecclisse Totale di
Sole del 22 Dicembre, 1870, eseguite in Sicilia dalla
Commissione Italiana, folio. *Palermo, 1872*
- The
Observatory,
Palermo. Palermo, Bulletino Meteorologico, vol. viii., 1872, folio.
Palermo, 1872
- The
Institute. ———, Istituto Tecnico, Giornale di Scienze Naturali ed
Economiche, vol. ii., fasc. 1, 4to. *Palermo, 1866*

- Paris Observatory, *Bulletins Astronomiques*, v. n., 8vo. The
Paris, 1872 Observatory.
- , *Rapport présenté à la Commission d'In-* ———
spection par le Directeur le 31 Mai, 1872, 4to.
Paris, 1872
- Petersbourg, St., Imperial Academy, *Mémoires*, vii serie, tome The Imperial
xvii., Nos. 1 to 10; tome xviii., Nos. 3 to 5, 4to. Academy.
St. Petersburg, 1871-72
- , *Bulletin*, tome xvii., folio. ———
St. Petersburg, 1871-72
- Observatory, *Jahresbericht am 27 Mai, 1871,* M. O. von
dem Comite der Nicolai Hauptsternwarte, 8vo. Struve.
St. Petersburg, 1871
- Plantamour, E., and Hirsch, A., *Nivellement de précision de* Prof.
la Suisse, 4to. Genève, 1872 Plantamour.
- , *Détermination Télégraphique* The Authors.
de la différence de Longitude entre les Stations Suisses,
4to. Genève et Basle, 1872
- Prag. *Magnetische und Meteorologische Beobachtungen auf* The
der Sternwarte im Jahre 1871, 4to. Prag, 1872 Observatory at
Prag.
- Quetelet, Ad., *Tables de la Mortalité et leur développement,* The Author.
4to. Bruxelles, 1872
- , *Unité de l'Espèce Humaine*, 12mo. ———
Bruxelles, 1872
- , *Notice sur C. Babbage*, 12mo. Bruxelles, 1872 ———
- Radcliffe Observatory, *Astronomical and Meteorological Obser-* The Radcliffe
vations in the year 1869, vol. xxix., 8vo. Oxford, 1872 Trustees.
- Respighi, Prof., *Sulle Osservazioni Spettroscopiche del Bordo e* The Author.
delle Protuberanze Solari fatte al Osservatorio della Uni-
versita Romana sul Campidoglio, 4to. Roma, 1872
- , *Sulla Nota dal Prof. P. Secchi intitolata* ———
"Sull' ultima Eclisse du 12 Décembre, 1871," 4to. 1872

- The Author. Robertson, F., Engineering Notes, 8vo. *London, 1873*
- The Collegio Romano. Rome, Collegio Romano, Bulletino Meteorologico, June 1872 to Feb. 1873, folio. *Rome, 1872-73*
- The Society. Royal Society, Philosophical Transactions, vol. clxii., part 1, 4to. *London, 1872*
- Proceedings, Nos. 135 to 144, 8vo. *London, 1872-73*
- Catalogue of Scientific Books, vol. vi., 4to. *London, 1872*
- Report of the Meteorological Committee for the year ending 31 Dec., 1871, 8vo. *London, 1872*
- J. M. Wilson, Rugby School Natural History Society, Report for 1872, 8vo. *Rugby, 1873*
- The American Academy. Rumford, Count, Complete Works, vol. i., 8vo. *Boston, 1870*
- The Observatory at San Fernando. San Fernando, Almanaque Nautico para 1874, 8vo. *Cadiz, 1872*
- The Society. Saxon Society of Sciences, Berichte uber die Verhandlungen, 1871 to Dec. 1872, 8vo. *Leipzig, 1872*
- The Authors. Schiaparelli, G. V., and Denza, P. F. Sulla grande Pioggia de Stelle Cadenti prodotta dalla Cometa de Biela osservata le sera del 27 Nov. 1872, 8vo. *Milano, 1872*
- The Society at Bern. Schweirische Meteorologische Beobachtungen, for 1871, 4to. *Berne, 1872*
- The Author. Secchi, Prof., Sulla distribuzione della Protuberanza Solare, 7th and 8th notice, 4to. *Roma, 1872*
- , Note relative à l'Observation du Passage de Venus devant le Soleil au Moyen du Spectroscope, 4to. *Roma, 1872-3*
- , Note Spectroscopiche sul Sole, 8vo. *Roma, 1872*

- Semler, Christian Gottlieb, Vollständige beschreibung des Sterns der weisen, &c., mit Augustin Nathannel Grishows Faye register von den Cometen des 1743 sten Jahres, 8vo. *Halle, 1743* W. G. Littow, Esq.
- Smithsonian Institution, Annual Report for the Year 1870, 8vo. *Washington, 1871* The Institution.
- Steen, Adolf, Løeren om homogene tunge Vædskers Tryk paa plane Arcaler, 4to. *Kjobenhavn, 1872* The Author.
- Struve, Otto, von, Tabulæ quantitatum Besselianarum pro Annis 1875 ad 1879 computatæ, 8vo. *St. Petersbourg, 1871* ———
- Sweden. Kongliga Svenska Vetenskaps Akademiens Handlingar, 1868-70, 4to. *Stockholm, v. y.* The Swedish Royal Academy.
- Ofversigt af do. 1869-70, 8vo. *Stockholm, 1869-70* ———
- Lefnad stickningar afver, do. Band i., Häfte 2, 8vo. *Stockholm, 1870* ———
- Sydney Government Observatory, Report of the Astronomer for 1871, folio. *Sydney, 1872* H.M. Government of N. S. Wales.
- , Meteorological Observations from February 1872 to January 1873, 8vo. *Sydney, 1872-73* ———
- , Results of ditto, ditto, in New South Wales during 1871, 8vo. *Sydney, 1872* ———
- Tasmania Royal Society, Monthly Notices, 1871, 8vo. *Hobart Town, 1872* The Society.
- Turin Royal Observatory, Bulletino Meteorologico ed Astronomico, an. vi., 4to. *Torino, 1872* The Observatory.
- Academy of Sciences, Atti, vol. vii., Nov. 1871 to June 1872, 8vo. *Torino, 1871-72* The Turin Academy.
- United States Coast Survey, Report of the Superintendent of the, showing the Progress of the Survey during the year 1868, 4to. *Washington, 1871* Prof. Benj. Pierce.

- Gen. A. J. Myer. United States of America, Annual Report of the Chief Signal Officer to the Secretary of War for the fiscal year ended June 30, 1871, 8vo. *Washington*, 1871
- The Upsala Royal Society. Upsala, Nova Acta regiæ Societatis, Ser. iii., vol. viii., part 1, 4to. *Upsaliæ*, 1871
- The Observatory at Upsala. ——— Bulletins Météorologiques mensuel de l'Observatoire de l'Université, vol. i., folio. *Upsala*, 1871
- S. M. Drach, Esq. Van Shooten, Tabulæ Sinarum Tangentium Secantium ad Radium 10,000,000. 12mo. *Amsterdam*, 1627
- The U. S. Government. Venus, Transit of 1874, Papers relating to the, prepared under the direction of the Commission authorised by Congress, and published by authority of the Hon. Secretary of the Navy, parts 1, 2, 4to. *Washington*, 1872
- H. M. Government for N. S. Wales. Victoria Observatory, Eighth Report of the Board of Visitors, with the Annual Report of the Government Astronomer, folio. *Melbourne*, 1872
- The Imperial Academy. Vienna Imperial Academy, Denkschriften, vols. 31, 32, 4to. *Wien*, 1872-73
- ———, Sitzungsberichte, vol. 64, 1871, Jan. to May, 1872, 8vo. *Wien*, 1871-72
- ———, Register zu dem Banden 61, bis 64, 8vo. *Wien*, 1872
- The Observatory. Vienna Observatory, Annalen, Dritter folge, Achtzehnter, Band 1868, 8vo. *Wien*, 1872
- The Author. Vogel, II. C., Bahn Bestimmung und oppositions Ephemeride der Antiope ⁽⁹⁰⁾, 4to. *Altona*
- ———, Bahn Bestimmung der Cometen v. 1846, 4to. *Altona*
- ———, Verzeichniss von hundert Nebelflicken, 4to. *Altona*
- ———, Bahnbestimmung und Ephemeriden der Planeten Antiope ⁽⁹⁰⁾ und Aigle ⁽⁹⁶⁾, 4to. *Altona*

- Vogel, H. C., Untersuchungen über das Spectrum der Nordlichts, 8vo. *Jena*, 1871 The Author.
- , Beobachtungen von Nebelflecken und Sternhaufen am Sechsfussigen refractor und Zwölf Fussigen Aequatoreal der Leipziger Sternwarte, 8vo. *Leipzig*, 1867
- , Ueber die Absorption des chemisch Wirksamen Strahlen in der atmosphäre der Sonne, 8vo. *Leipzig*, 1872
- , Resultate, spectralanalytischer Untersuchungen an Gestirnen, 8vo. *Leipzig*, 1871
- Wace, Rev. H., Simple method of Calculating Logarithms, 12mo. *London*, 1873
- Washington Observations for 1868, Appendix I., A Catalogue of 1963 Stars reduced to the beginning of the year 1850, together with a Catalogue of 290 Double-Stars; the whole from Observations made at Santiago, Chili, during the years 1850, 51, 52, by the U. S. Naval Astronomical Expedition to the Southern Hemisphere, Lieut. J. M. Gilliss, Superintendent, 4to. *Washington*, 1870 The U. S. Naval Observatory.
- for 1869, Appendix II., Zones of Stars observed at the U. S. Naval Observatory with the mural circle in 1846, 7, 8, 9, by Prof. J. C. H. Coffin and others, 4to. *Washington*, 1872
- Weisse, Prof. E., Bestimmung der langen differenz Wien, Wiener, Neustadt durch chronometer Übertragungen, 8vo. *Wien*, 1872 The Author.
- , Bestimmung der Breite und des Azimuths auf dem laaerberge bei Wien, 8vo. *Wien*, 1871
- Weiler, A., Grundzüge eines neuen Störungstheorie und deren anwendung auf der Theorie des Mondes, 4to. *Leipzig*, 1872
- Wijkander, August, Beräkning af Planeten (117) Lomias bana, 4to. *Lund*, 1872

- The Author. Wilkinson, T. T., *De Tactionibus, or some Problems on Contact*, 8vo. *Liverpool*, 1873
- Williams, W. M., *The Fuel of the Sun*, 8vo. *London*, 1870
- Winnecke, F. A. T., *Bestimmung der Parallaxe des zweiten Argelandischen Sternes*, 4to. *Leipzig*, 1872
- Wolf, Dr. R., *Handbuch der Mathematik Physik Geodäsie und Astronomie, Zweiter Band*, 8vo. *Zurich*, 1872
- ———, *Astronomische Mittheilungen*, Nos. 21 to 31. 8vo. *Zurich*, 1872
- ———, *Beiträge zur Geschichte der Schweizer Karten*. 4to. *Zurich*, 1873
- Young, C. A., *Catalogue of bright Lines in the Spectrum of the Solar Atmosphere*, 8vo. *Philadelphia*, 1872
- Young, Dr. E., *Special Report on Immigration, accompanying information for Immigrants relative to the Prices and Rentals of Land, &c. &c.*, 8vo. *Washington*, 1872
- The Society. Yorkshire Geological and Polytechnic Society, *Proceedings*, n. s., pt. 1, 8vo. *Leeds*, 1871-2
- Zollner, F., *Zur Geschichte des Horizontal Pendels*, 8vo. *Leipzig*, 1872
- ———, *Ueber die durch stromendes Wasser erzeugten electrischen Ströme*, 8vo. *Leipzig*, 1872
- ———, *Ueber das Spectroscopische reversionssfernrohr*, 8vo. *Leipzig*, 1872
- ———, *Ueber die Zusammenhang von Sternschnuppen und Cometen*, 8vo. *Leipzig*, 1872
- ———, *Ueber die Electriche und Magnetische Fernwirkung der Sonne*, 8vo. *Leipzig*, 1872
- The Society. Zoological Society, *Transactions*, vol. viii., parts 2 to 5, 4to. *London*, 1872-73
- ———, *Proceedings*, 1871, Nos. 1 to 3; 1872, part 2 and Index, 8vo. *London*, 1872-73

Zoological Society, Revised List of Vertebrated Animals now The Society.
or lately living in the Gardens of the Society, 8vo.

London, 1872

—————, Catalogue of the Library of, 8vo. ———

London, 1872

Zurich, Naturforschenden Gesellschaft Vierteljahrsschrift, ———
16th year, 4 parts, 8vo. *Zurich, 1871*

Miscellaneous.

An Engraving of R. S. Newall, Esq.'s large Telescope. R. S. Newall,
(Framed.) Esq.

Chart of the Northern Heavens on an equal-surface projection, The Author.
showing at one view all the Stars (324,198) in Arge-
lander's Series of 40 Charts, by R. A. Proctor.

A Photograph of the Moon on Glass.

J. Browning,
Esq.

An Observing Chair.

H. W. Hollis,
Esq.

Various Maps and Charts.

The Dépôt
Gen. de la
Marine.

A fine Engraving, framed, of the late Sir James Clark Ross.

Admiral
Ommanney.

Sextant by Bird, said to have been the property of Capt. Cook. Dr. Radford-

